THE AMERICAN PSYCHOLOGIST

VOLUME 15 NUMBER 11

November 1960

Published Monthly by

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

THE AMERICAN PSYCHOLOGIST

Journal of the American Psychological Association, Inc.

Volume 15 November 1960 Number 11

In this Issue

JOHN G. DARLEY, Editor

ARTHUR C. HOFFMAN, Managing Editor HELEN ORR, Promotion Manager

THE AMERICAN PSYCHOLOGIST is published monthly at Prince and Lemon Streets, Lancaster, Pennsylvania. Subscription: $8.00, single copy: $1.00. Communications on editorial and business matters and advertising should be addressed to the American Psychological Association, Inc., 1333 Sixteenth Street N.W., Washington 6, D. C. Address changes must reach the Subscription Office by the 10th of the month to take effect the following month. Undelivered copies resulting from address changes will not be replaced; subscribers should notify the post office that they will guarantee second-class forwarding postage. Other claims for undelivered copies must be made within four months of publication.

Second-class postage paid at Lancaster, Pa.

Officers of the American Psychological Association

President

NEAL E. MILLER
Yale University
New Haven, Connecticut

President-elect

PAUL E. MEEHL
University of Minnesota
Minneapolis, Minnesota

Past President

DONALD O. HEBB
McGill University
Montreal, Canada

Recording Secretary

LAUNOR F. CARTER
System Development Corporation
Santa Monica, California

Treasurer

MEREDITH P. CRAWFORD
Human Resources Research Office
Washington 6, D. C.

Executive Secretary

JOHN G. DARLEY
1333 Sixteenth Street N.W
Washington 6, D. C.

Board of Directors

The President, the President-elect, the Past President, the Recording Secretary, the Treasurer, the Executive Secretary, and DANIEL KATZ, QUINN MCNEMAR, EDWIN B. NEWMAN, VICTOR C. RAIMY, NEVITT SANFORD, and M. BREWSTER SMITH.

The American Psychological Association, founded in 1892 and incorporated in 1925, is the major psychological organization in the United States. The purpose of the APA is to advance psychology as a science, as a profession, and as a means of promoting human welfare. It attempts to further these objectives by holding annual meetings, publishing psychological journals, and working toward improved standards for psychological training and service.

In addition to journals, the APA publishes a monthly *Employment Bulletin* and an annual biographical *Directory*.

All general communications; inquiries concerning membership; letters concerning dues, subscriptions, and changes of address should be directed to:

American Psychological Association, Inc.
1333 Sixteenth Street, N.W.
Washington 6, D. C.

Publications of the American Psychological Association

American Psychologist. Editor: JOHN G. DARLEY, American Psychological Association. Official papers of the association and articles on psychology. Monthly. Subscription: $8.00 (Foreign $8.50). Single copies, $1.00.

Contemporary Psychology. Editor: EDWIN G. BORING, Harvard University. Critical reviews of books, films, and other material in the field of psychology. Monthly. Subscription: $8.00 (Foreign $8.00). Single copies, $1.00.

Journal of Abnormal and Social Psychology. Editor: M. BREWSTER SMITH, University of California, Berkeley. Original contributions in the field of abnormal and social psychology, and case reports. Bimonthly, two volumes per year. Subscription: $16.00 for 2 vols. (Foreign $16.50). Single copies, $3.00.

Journal of Applied Psychology. Editor: JOHN G. DARLEY, American Psychological Association. Applications of psychology to business and industry. Bimonthly. Subscription: $8.00 (Foreign $8.50). Single copies, $1.50.

Journal of Comparative and Physiological Psychology. Editor: HARRY F. HARLOW, University of Wisconsin. Original contributions in the field of comparative and physiological psychology. Bimonthly. Subscription: $8.00 (Foreign $8.50). Single copies, $1.50.

Journal of Consulting Psychology. Editor: EDWARD S. BORDIN, University of Michigan. Research in clinical psychology: psychological diagnosis, psychotherapy, personality, and psychopathology. Bimonthly. Subscription: $8.00 (Foreign $8.50). Single copies, $1.50.

Journal of Educational Psychology. Editor: RAYMOND G. KUHLEN, Syracuse University. Studies of learning and teaching. Bimonthly. Subscription: $8.00 (Foreign $8.50). Single copies, $1.50.

Journal of Experimental Psychology. Editor: ARTHUR W. MELTON, University of Michigan. Original contributions of an experimental character. Monthly, two volumes per year. Subscription: $16.00 for 2 vols. (Foreign $16.50). Single copies, $1.50.

Psychological Abstracts. Editor: HORACE B. ENGLISH, Ohio State University. Noncritical abstracts of the world's literature in psychology and related subjects. Bimonthly. Subscription: $16.00 (Foreign $16.50). Single copies, $3.00.

Psychological Bulletin. Editor: HARRY HELSON, University of Texas. Evaluative reviews of research literature, and articles on research methodology in psychology. Bimonthly. Subscription: $8.00 (Foreign $8.50). Single copies, $1.50.

Psychological Monographs: General and Applied. Editor: NORMAN L. MUNN, Bowdoin College. Longer researches and laboratory studies which appear as units. Published at irregular intervals. Subscription: $8.00 per volume (Foreign $8.50). Single copies, price varies according to size.

Psychological Review. Editor: RICHARD L. SOLOMON, Harvard University. Original contributions of a theoretical nature. Bimonthly. Subscription: $8.00 (Foreign $8.50). Single copies, $1.50.

COMPUTER SIMULATION OF THINKING[1]

CARL I. HOVLAND[2]

Yale University

IT is commonplace in the history of science for developments in one field of knowledge to have profound effects on other related areas. The dramatic influence of advances in atomic physics on biology, genetics, and medicine is a good case in point. We are currently witnessing a similar phenomenon in the repercussions of high speed computer technology on research in the behavioral sciences. The initial impact came from the computational efficiency of these devices which permitted calculations formerly prohibitive in terms of time and effort. A more recent and less direct effect has been in stimulating machine-like methods of analysis of human thought and behavior through simulation on high speed computers. It is these newer techniques and their applicability to psychological problems that is the topic of the present paper.

The analogy between the high speed computer and human thinking has long been noted. We frequently see the Univacs, Johniacs, Illiacs referred to in the popular press as "giant brains" or "thinking machines." In most uses of high speed computers, however, there is an attempt to attain objectives beyond the scope of human capabilities, either because of their speed or their extensive storage capacity (called, interestingly enough, their "memory"). But in the investigations I shall be describing, the utilization is quite different. Here we are primarily concerned with the use of computing machines to simulate in exact fashion the way a human solves a problem. Both human weaknesses, such as limited and fallible memory, and strengths, such as the ability to choose an efficient solution out of innumerable alternatives, must be represented. We say that we can simulate human problem solving when we are able to specify both the prior information a human possesses and the sequence of steps by which he utilizes this information in the solution of the problem. We are then able to set up a computing machine to carry out this same sequence of operation.

Those familiar with the operation of high speed computers will readily understand the way in which simulation proceeds. Just as in ordinary operations of a computer, one gives the machine a set of "instructions" to execute. These constitute a "program." In arithmetical operations these are sentences like the following: "square the product of the first and second number," "store the product in memory," "compare the first and second number," "select the larger of the two numbers compared." Or such instructions as: "find the number of dollars paid to the individual last month," "add to this amount the number of dollars earned this month," and so forth. The machine then executes each of these instructions through an intricate electronic system, printing out its answers on an electric typewriter. Sequences of instructions can then solve the most complicated numerical problems, such as making out a payroll with each individual working different numbers of hours, at different wage rates, with advance payments to some workers, with different deductions for subscriptions to health and accident insurance, different income tax credits, and so forth. The nub of the simulation problem involves the use of similar types of "programs" of "instructions" to the machine in order to reproduce the steps an individual goes through in thinking out the solution to a difficult problem. One specifies the steps the individual uses by stating them in an unambiguous way so that a computing machine is able to carry them out. These may be instructions like: "store the answer to the last problem," "determine whether you have stored in memory any similar problems," "if so, what are the differences between the past problem and the present problem," "see if applying Rule a will convert the old problem into the new one," and "apply Rule b" to convert the answer to the former problem into the solution

[1] Adapted from a talk given over the *Voice of America* in September 1959. Unrestricted use of this material is available to the United States Government without cost.

[2] The author is a member of the Social Science Research Council Committee on Simulation of Cognitive Processes. This committee supports relevant work in this field and welcomes suggestions for fellowships or research projects from interested investigators.

to the present one. Thus the computer can be given information which is exactly equivalent to that of the human problem solver, as well as a specification of the way the human goes about processing that information to reach a solution.

The obvious point is that if we can be precise enough about a process to describe it in terms which can be programed and executed by a machine, we indeed know quite a bit about that process. And if we can specify singly each of the subprocesses involved, we can determine the effects of combinations of them and of variations in order of execution of the steps. The outcomes are almost impossible to foresee without actually carrying out the combinations and variations.

Let me begin by giving a concrete example of the new techniques, namely, simulation of the solving of geometry problems. We certainly think of the solving of theorems in Euclidian geometry by a high school sophomore as constituting a clear-cut example of intelligent human behavior. But Gelernter and Rochester (1958) of the International Business Machines Company have now successfully developed a program whereby a high speed computer is able to solve many of the theorems in Euclid's geometry, for example, that the diagonals of a parallelogram bisect one another. A human learner who tries to solve such a problem has usually been taught a series of fundamental principles, or axioms, together with a set of rules for inferring relationships by which the basic symbols in the system may be manipulated. He is then asked to prove a new theorem. He tries to find a way of transforming and combining previous axioms through the set of rules until he achieves the proof of the new theorem. Typically he starts out in rather routine fashion, then has a flash of insight as to a possible means of solution, and then methodically tests the adequacy of the solution. The geometry computing machine is set up to operate in an analogous fashion. It is given a set of basic formulas and axioms, together with rules as to possible ways of manipulating them in order to form new theorems. The new theorem is then presented to the machine to prove. The machine is equipped with a number of rules of thumb for possible ways of solving problems. For example, it is instructed that if the proposition to be proved involves parallel lines and equality of angles, there is a good chance that it may be useful to try the theorem: "If two parallel lines are intersected by a third line, the opposite interior angles are equal." This instruction constitutes a short-cut which often works well but is by no means sure to be of value. Successful solution typically involves setting up a series of subgoals which are then worked on in succession. For example, in the problem cited earlier the machine ascertains that it can solve the theorem if it can establish the fact that the distance from one corner of the base of the parallelogram to the point of intersection must equal the distance from the intersection to the opposite corner of the parallelogram. This is then a subgoal, which in turn can be proved if the triangle formed by the bisecting lines and one of the sides of the parallelogram is equal to the triangle formed by the opposite side and the corresponding bisects. A device is incorporated into the computer which makes constructions and measures lines and angles. This operates by means of coordinate geometry. Once the sequence of subgoals leads from the initial axioms to the theorem to be proved, the machine routinely tests the accuracy of the proof. This it can do in an exhaustive manner, since once one has a possible proof, checking it is largely clerical. The chief problem is to find a possible method of proceeding, out of the almost infinite number of alternatives. It is here that the short-cut methods operate. They permit the use of likely and plausible methods of solution, just the way a clever high school student would proceed. Once the proof has been verified, the machine prints QED. Throughout the entire operation the machine prints out on paper a complete tracing of the steps it tries—this is analogous to an individual's account of the way he solves a problem in geometry. Some of the machine's failures in finding proofs closely resemble those made by beginning geometry students.

It will be noted that the methods of solution built into the computer closely resemble those used by humans solving similar problems. Let me again call attention to the fact that in this way they differ from the usual uses of high speed computers which methodically go through every possible solution in a deliberate way. The complete methods guarantee that if there is a solution it will be found, although an extraordinary number of trials may be required. Solutions of this type are referred to as "algorithms." These are used here to check proofs. In contrast, finding a possible solution is facilitated by short-cuts and rules of thumb

programed into the machine. In this way it simulates a human subject in making leaps in the solution and trying out schemes which have been successful in the past, rather than exhaustively trying out each possible alternative. Mathematicians call these short-cut solutions "heuristics."

One may wonder whether we have gained anything by the simulation since we initially derive processes from study of how students work and then program into the computer their ways of proceeding. In fact, at the outset, we may operate in a somewhat circular fashion—that is, we may only get out of the machine what we put into it. But as one proceeds, new combinations are tested which could not have been predicted from the individual steps. Some results, although strictly determined by the processes programed, are impossible to foresee because so many complex operations interact in the final solution. One can find out the effect of increased complexity of problems, and then determine with human subjects whether the order of difficulty is the same that would be predicted from the computer's information processing routines. In this way one is constantly working back and forth from experiments with human subjects to simulation on the computing machine. Furthermore one frequently finds that one must make assumptions about certain steps in the process to get the computer to execute its program correctly. Here the simulation comes first and suggests later experiments with human subjects.

The geometry machine just described involves solving problems rather than learning how to solve them, in the sense that the computer would solve the same problem in the same way on a second trial. Humans, of course, do learn and improve through practice. So the interesting task is to build into the computer this capability as well. Simulation of learning is one of the most interesting potential applications of computer simulation techniques, since the ability to learn is one of the clear-cut differences between human and machine performance. A number of different types of learning are currently being simulated. The first involves stimulus-response learning. It is rather simple to simulate this type of learning with rewards ("reinforcements") given when certain types of behavior occur and not given when other types of responses are made. The probability that the response followed by reward will occur on later trials can then be made to increase. Failure of reward, or punishment, can be made to lead to a decreased probability of response ("extinction"). The studies of Herman, a computing machine, carried out by Friedberg (1958), and of the Perceptron, investigated by Rosenblatt (1958), are interesting examples of artificial learning machines. Other related possibilities are discussed in Miller, Galanter, and Pribram (1960).

At a somewhat more complex level is the type of learning involved in recognizing patterns imbedded in complex stimuli. It seems a simple thing for a human to respond to a triangle as a triangle whether it is large or small, short or tall, tilted or upright, and to distinguish it clearly from a square. But to specify rigorously the criteria in such a way that a machine can learn to recognize it invariably is quite a job. And the difficulty clearly hints that there is a lot we do not understand about the phenomenon even at the human level where we take the process for granted. Selfridge (1955) and Dinneen (1955) have worked most extensively on this problem and have been able to develop methods for getting the salient features of patterns to stand out so that some uniform response is given to a particular pattern. With two techniques, one of "averaging," to get rid of random elements, and a second, of "edging," to maximize the most distinctive features, they are able to insure that a variety of different ways of writing the letter A, for example, are registered as the same letter in the computer as a basis for further processing.

The third type of learning is made possible by keeping records of success and failure attained when different methods are pursued and using these records to improve performance. Thus, in the case of the geometry computer it is possible to store theorems which have already been proved. Similar mechanisms have been incorporated into the General Problem Solver developed by Newell, Shaw, and Simon (1958). It is also possible for these machines to be selective in their choice of theorems for permanent storage, rejecting those which do not seem sufficiently general to be useful later on. The most highly developed simulation of this type of learning is that incorporated in a checker-playing machine developed by Samuel (1959). His machine utilizes a type of rote learning which stores all of the checkerboard positions it encounters in play, together with the outcomes

following each move. In addition this machine has some capacity to generalize on the basis of past experience and to store the generalizations themselves. With these learning mechanisms it appears possible for the computer to learn in a short period of time to play a better game of checkers than can be played by the person who wrote the program.

Many of the formulations of learning are made without any special assumptions that learning processes are consistent with known neurophysiological mechanisms. A number of students are attempting to close this gap by simulation studies of the way in which nerve networks become organized into systems and are then modified through use. There is quite extensive investigation along these lines, some of it instigated by the speculations of Hebb about the nature of nervous organization. Suffice it to say that a number of researchers have been able to program computers to simulate the changing of neural organization patterns as a result of repeated stimulation of nerve fibers and further work of a similar type is in progress (cf. Clark & Farley, 1955, and Rochester, Holland, Haibt, & Duda, 1956).

In the work in our laboratory the emphasis is on understanding and simulating the processes involved in acquiring complex concepts through experience (Hovland & Hunt, 1960). The learner acquires a particular concept when he is told which of a series of specific instances presented to him belong in the concept class and which do not. This is similar to the way in which a child learns the concept of "animate" through some experiences in which parents and teachers label a given stimulus as "animate" and others in which they label it as "inanimate" (Hovland, 1952).

Our type of problem is illustrated by a situation in which there are a large number of slides of cancer cells, some of which are known to be malignant and others nonmalignant. The task of the individual (or the machine) is one of inducing the base of difference between the two types and subsequently labeling correctly new slides previously unidentified. Medical pathologists have just such a task and have achieved considerable success, although not 100% accuracy, in making such distinctions. It is of interest in passing that there is a machine available which can make such a distinction on the basis of slides presented to it, but here the combination of characteristics (the "concept") was formulated by the scientist who developed the instrument (Tolles & Bostrom, 1956). The machine's task is to see whether the new specimen conforms to certain specifications, that is, whether on the basis of density and structure the cell belongs in the "malignant" or "normal" category. Thus it has the "concept" built into it, obviating the need to start from the beginning in order to induce it.

The input to the type of concept learning in which we are interested is a series of pictures, say flower designs (Hovland, 1953), some of which are labeled "positive" instances (examples of the concept) and some "negative" instances (examples of what the concept *is not*). The characteristics of the instances are represented as symbols for processing by the machine. It is hoped later to have this transformation automatic through the use of techniques developed at the Bell Telephone Laboratories which employ a television camera to convert the visual representation into electrical impulses as input to the computer. Thus the picture would become converted into one set of symbols representing the characteristics which constitute the instances of the concept (like A1B2C1D1E2F1G1H2), while another string of symbols will represent instances of what the concept *is not* (like A2B1C1D2E1F1G1H2).

Potentially, a machine can then consider combinations of all of these characteristics as possible ways of categorizing and distinguishing between the class of "A" and of "not A." Typically, human learners only attend to part of the potential set of characteristics because of perceptual limitations. We have devoted considerable research effort toward determining just how attention and perception vary during the course of learning. We have incorporated in the machine simulation a selective scanning of possible aspects of the complex stimuli with provision for the fact that some individuals see only some of the characteristics while other individuals pay attention to different aspects.

Human subjects, at least at the adult level, operate on material of this type by developing strategies involving some generalization as to what concepts are like. Some details of these strategies have been investigated by Bruner, Goodnow, and Austin (1956). The strategies may be different for different types of concepts. Logicians describe some concepts as being of the *conjunctive* type, where all the members of the class share certain common

characteristics. For example, rubies share the characteristics of hardness, translucence, and redness. A second type of concept is called *disjunctive*, in which possession of either one characteristic or possession of a different characteristic makes the instance subsumable under the general class. This is illustrated by the concept of "strike" in American baseball which is either a pitched ball across the plate and between the batter's knees and shoulders *or*, alternatively, any pitch at which the batter strikes but fails to send into the field. A third type of concept is *relational*, where the instances of the concept share no common fixed characteristics but do have certain relationships in common. A sample would be the concept of "isosceles triangles." All instances of this concept involve triangles with two equal sides. But any fixed characteristics, such as lengths of the equal sides, lengths of the third side, or sizes of angles, are not an adequate basis for inclusion or exclusion in the concept class.

In preparation for later simulation, we have carried out extensive experimentation to determine the order in which these various types of concepts are considered by human learners. We find that for our type of stimulus materials, conjunctive and relational concepts are considered much more commonly than disjunctive ones (Hunt & Hovland, 1960). So our present machine will have built into it a hierarchy of responses in which the first attempts to organize the material will be in terms of shared characteristics—conjunctive type concepts. Alternatively the machine will consider concepts which are based on relationships between the stimuli. Only when these have been extensively and unsuccessfully explored will the machine try disjunctive concept patterns.

At present, then, we have the program for a machine which is able to receive drawings having a number of different dimensions. It is then able to try a number of possible ways of organizing into a concept the prior information it has received regarding confirming and nonconfirming instances. First it considers possibilities of concepts which have various combinations of features. When none of these suffice, it considers relational concepts. When these are not successful, it considers various disjunctive concepts where one set of features or another alternative set define the concept. When a solution is reached the description of what constitutes a concept is printed out on tape and subsequent unlabeled instances are classified A's or non-A's. A scanning device is built into the machine to take into account only certain of the characteristics available for consideration. The present machine remembers all that has been presented to it. We are currently considering various devices to simulate the gradual loss of information, or forgetting, which is all too human a characteristic. Our experimental studies have indicated the overall mathematical form which the loss should take, but there are alternative means of producing such a loss (Cahill & Hovland, 1960). Each alternative represents a different theory of the way in which forgetting occurs and investigation of the different theories is of fundamental importance. Simulation again provides a powerful tool for specifying the operation of the process of forgetting.

A high proportion of our research effort goes into new experimentation with human learners to determine their methods of handling various aspects of the problem, as compared to other efforts which stress programing the actual simulation. It is expected that this type of imbalance in effort will continue, but we are perennially hopeful that as more and more information becomes available an increasing amount of our effort will go into the simulation itself.

Work has now progressed to the point where I think we can see more clearly both the opportunities provided by these methods and some of the difficulties involved. I hope that the foregoing discussion has suggested some of the advantages of these new techniques. Let me briefly summarize the potentialities. First, simulation methods have a tremendous role in sharpening our formulations concerning mental processes and phenomena. It is one thing to say, as earlier students have said, that problem solving involves a number of different stages, for example, those of preparation, incubation, illumination, and verification, and quite another thing for one to specify exactly what is involved in each stage. The pioneering studies by Newell, Shaw, and Simon (1958) on the General Problem Solver indicate the great forward strides which result from specifying the nature of these processes in such complete detail that a computer is able to solve problems by following the sequence of steps programed into the machine.

Closely related is the second advantage of the

computer, the emphasis which it places on developing theories that have both descriptive and predictive power. Many of the theories which exist in psychology and sociology are so general and vague that they have little real predictive power. The program written for the computer to describe a particular process constitutes a theory which, if successful in carrying out the process in the same way as the human, is highly efficient in predicting the effects of changes in conditions and in specifying what other individuals will do under particular conditions.

Lastly, the simulation of human responses has the same overwhelming advantages for our understanding of behavioral phenomena as similar methods in other sciences. For example, the use of the wind tunnel represents a complex set of interacting conditions in actuality which could not be duplicated and whose effects could not be predicted from theory alone. Analogously in the present case, for single factors one can analyze effects without simulation, but when one seeks to understand the combined action of a number of factors interacting in complex ways, no satisfactory way of predicting the exact outcome may be possible. Those working on the geometry simulator, the General Problem Solver, and the chess and checker-playing machines, all testify to the fact that many of the moves made by the computer greatly surprised their inventors.

I hope that my remarks on the importance of simulation methods do not give rise to the feeling that these methods automatically lead to quick success in areas which have been investigated for decades using other techniques. Two examples of the difficulties confronting us may be mentioned. The first is the complexity of the process to be simulated. At present we consider ourselves fortunate if we can simulate on a machine the typical performance of a single individual in solving a particular problem. This is indeed a great step forward. But for simulation to be maximally effective we would like to be able to predict machine solutions which simulate not only a single individual under some specified condition, but also the effects for different individuals under different environmental conditions, and after various amounts of experience. To date, most simulation has been of the performance of one individual, either real or an imaginary average individual. It may prove to be extremely difficult to carry out the next step, that of specifying which characteristics must be known about each individual to be able to simulate the way he varies from the typical pattern. In addition, the effects of environmental variables, such as the effects of drugs on performance, or of pressure to complete a task, should then be simulated. Finally, the effects of experience should be specified, so that the way in which a problem is attacked is appropriately changed as a result of the machine's ability to learn. This leaves for the future such a complex problem as analysis of the interactions between type of individual and amount of learning under different environmental conditions. It is apparent that a long and difficult road lies ahead before we can accomplish successful simulation of a single type of task which has all of these variables programed. But when they can be successfully specified we will know a great deal about the problem. Most research generalizations in the social sciences are only true for a group of people, not for each individual. Computer methodology may make possible a broadening of our understanding of behavior by emphasizing the simulation of single individuals and then studying variations between them. The integration of these complementary approaches in new computer work will help us to reduce the gap between group averages and individual processes.

A second example of the difficulties of machine simulation is attributable to the nature of the process with which we are concerned. Simulation methods have most successfully been employed where it is possible to define the final performance of a task as an outcome of a succession of single steps. Thus where the mental process involves steps in a sequence one can synthesize the process by having the computing machine work first on stage one, then stage two, etc. Much more difficult are those processes where a number of stages are going on simultaneously, in parallel fashion. It certainly appears that much of our perceptual and thought process operates in this way. Under these conditions it is much more difficult to untangle the processes at work prior to simulation. In addition, present machines are not as suitable for these purposes as they are for sequential operation. New and radically different machines may ultimately be required to cope with this problem. Most of our present work is being carried out with computers which were built for quite other purposes, namely, high speed arithmetical computation. It would be possible to design machines more closely simulating

thought processes and more flexible in their operation, but they would be expensive to construct and would not have the large number of potential purchasers who ordinarily help defray the costs of development.

Despite the difficulties mentioned, work on simulation of complex psychological processes is yielding results of increasing importance. Processes which were thought to be understood turn out to require much more explicit statement. But along with the increased explicitness comes new understanding and precision. At present most computer programs grapple with only one phase of complex processes, but we are beginning to see common features in a number of different programs, permitting the construction of comprehensive programs from simpler subprograms. Work on simulation has also had a stimulating effect on research on the higher thought processes themselves. Attempts to program computers have repeatedly revealed that we lacked much information as to how humans carry out seemingly simple thought operations. This has led to the return of workers to the laboratory which in turn has further enriched our knowledge of the human thought process.

Let not this enthusiastic report on the scientific potentialities of simulation research arouse anxieties of the sort raised by Norbert Wiener (1960) and other writers that machines will take over our civilization and supplant man in the near future. Rather, I think, there is great hope that detailed knowledge of how humans learn, think, and organize will redound to human welfare in removing much of the mystery which surrounds these processes and in leading to better understanding of the limitations of current ways of solving problems. It may, of course, become possible for us to then build machines which will work out solutions to many problems which we now consider distinctively human and to do so in a manner surpassing present human performance. But that this will lead to the machine becoming master and the designer, slave, seems to me most unlikely. Rather it will free man for novel creative tasks which are progressively beyond the capability of machines designed by man.

REFERENCES

BRUNER, J. S., GOODNOW, JACQUELINE, J., & AUSTIN, G. A. *A study of thinking.* New York: Wiley, 1956.

CAHILL, H., & HOVLAND, C. I. The role of memory in the acquisition of concepts. *J. exper. Psychol.*, 1960, **59**, 137–144.

CLARK, W. A., & FARLEY, B. G. Generalization of pattern recognition in a self-organizing system. In, *Proceedings of the Western Joint Computer Conference.* Institute of Radio Engineers, 1955. Pp. 86–91.

DINNEEN, G. P. Programming pattern recognition. In, *Proceedings of the Joint Western Computer Conference.* Institute Radio Engineers, 1955. Pp. 94–100.

FRIEDBERG, R. M. A learning machine. Part I. *IBM J. Res. Develpm.*, 1958, **2**, 2–13. (Cf. also 1959, **3**, 282–287.)

GELERNTER, H. L., & ROCHESTER, N. Intelligent behavior in problem-solving machines. *IBM J. Res. Develpm.*, 1958, **2**, 336–345.

HOVLAND, C. I. A "communication analysis" of concept learning. *Psychol. Rev.*, 1952, **59**, 461–472.

HOVLAND, C. I. A set of flower designs for concept learning experiments. *Amer. J. Psychol.*, 1953, **66**, 140–142.

HOVLAND, C. I., & HUNT, E. B. Computer simulation of concept attainment. *Behav. Sci.*, 1960, **5**, 265–267.

HUNT, E. B., & HOVLAND, C. I. Order of consideration of different types of concepts. *J. exper. Psychol.*, 1960, **59**, 220–225.

MILLER, G. A., GALANTER, E., & PRIBRAM, K. H. *Plans and the structure of behavior.* New York: Holt, 1960.

NEWELL, A., SHAW, J. C., & SIMON, H. A. Elements of a theory of human problem solving. *Psychol. Rev.*, 1958, **65**, 151–166.

ROCHESTER, N., HOLLAND, J. H., HAIBT, L. H., & DUDA, W. L. Tests on a cell assembly theory of the action of the brain, using a large digital computer. *Trans. Info. Theory*, 1956, **IT-2**(3), 80–93.

ROSENBLATT, F. The perceptron: A probabilistic model for information storage and organization in the brain. *Psychol. Rev.*, 1958, **65**, 386–408.

SAMUEL, A. L. Some studies in machine learning using the game of checkers. *IBM J. Res. Develpm.*, 1959, **3**, 211–229.

SELFRIDGE, O. G. Pattern recognition and modern computers. In, *Proceedings of the Western Computer Conference.* Institute of Radio Engineers, 1955. Pp. 91–93.

TOLLES, W. E., & BOSTROM, R. C. Automatic screening of cytological smears for cancer: The instrumentation. *Annals NY Acad. Sci.*, 1956, **63**, 1211–1218.

WIENER, N. Some moral and technical consequences of automation. *Science*, 1960, **131**, 1355–1358.

THE CONCEPT OF THE STIMULUS IN PSYCHOLOGY[1]

JAMES J. GIBSON

Cornell University

IT seems to me that there is a weak link in the chain of reasoning by which we explain experience and behavior, namely, our concept of the stimulus. The aim of this paper is to find out what psychologists mean by the term stimulus, with the hope of deciding what they *ought* to mean by it. After a short look at the history of the term, I will try to uncover the sources of confusion in modern usage. In the end, perhaps, the concept will be clarified. If not, certain contradictions will have been brought to light.

The experimental study of the stimulus began in the eighteenth century, so far as I can tell, with an investigation of the curious things that could be done to make a frog's leg twitch. The experimenters discovered what is now called the nerve-muscle preparation. Galvani and later Volta gave their names to electricity as well as to physiology by their experiments. In the early nineteenth century Johannes Müller applied these discoveries to the philosophers' problem of the human senses, the gates of knowledge. The nerves of sense, he pointed out, can be excited by a variety of unnatural agencies such as electrical current. Since the mind is acquainted only with the qualities specific to the sensory nerves, not with the stimuli, how it gets knowledge of the material world became more puzzling than ever. Later in the century, Sherrington was to emphasize the extent to which receptors are naturally protected against such irrelevant stimuli by the structural specialization of sense organs. But meanwhile it had been discovered that the skin would yield sensations only at certain discrete points. Here was a fresh puzzle. The separate receptor cells of all the sense organs came to be seen under the microscope, and the punctate character of the sensory process seemed to be established.

During all this time, the physical scientists were discovering the laws of energy and triumphantly measuring it in its various forms, electricity, momentum, light, heat, sound, and the results of chemical reaction. It became possible to measure certain variables of energy at sense organs, at least the simple ones like frequency and amount. Thresholds of reportable sensation were established. Fechner, following Weber, conceived the grand scheme of a measurement formula for consciousness, relating its judged intensity to a simple variable of the stimulus. Psychophysics was born.

Whatever could be controlled by an experimenter and applied to an observer could be thought of as a stimulus. In the growing science of human psychology, it became evident that this was the independent variable of an experiment, to be isolated and systematically varied. Much more complex things than physical energies could be presented to the sense organs—words for instance. These were also called stimuli, although the stimulus conditions manipulated, recency, frequency, meaningfulness, were vastly different from the variables of the psychophysical experiment.

In the latter part of the nineteenth century the concept of the reflex arc was applied to the adaptive behavior of animals. It had been thought to explain the strictly mechanical actions of the body ever since Descartes. Reflexes had stimuli. The situations of animals could be systematically altered and the reactions observed. Organisms obviously responded to such stimuli, and the experimenter could apply them more freely than he could venture to do with human beings. To shorten a long story, such experiments came to be merged with human experiments and the outcome was a general stimulus-response psychology. This was a great success, especially in America. But stimuli for animal psychologists were not the same as stimuli for sensory physiologists and stimuli were still different for the students of perception and learning.

Enough has been said to show that in the twentieth century we have inherited a mixed batch of ideas about the stimulus. We constantly use the word but seldom define it. We take it for granted. We have behavior theory in full bloom, and perception theory in ripened complexity, but who ever heard of stimulus theory? As a preliminary effort in this direction, I have made a survey of what

[1] Presidential Address to the Eastern Psychological Association, New York, New York, April 1960.

modern writers seem to mean by the term. Some writers define it, but not many. My method was to collect quotations from books. I then put them in opposition to one another. The ways of conceiving the stimulus are often in flat contradiction. Occasionally one book can be quoted against itself. The issues interlock, of course, but I have separated them into eight areas of disagreement and will treat them separately. In what follows, I will quote without comment, for the most part, keeping my own opinions to the end.

I. For Freud, the only use of the term stimulus that is discoverable in the *Collected Papers* (1949) is to refer to a motivating force. This, after all, is the dictionary meaning of the word—something that arouses or impels to action. In ordinary speech we refer to the stimulus of hunger or fear, which may compel extreme forms of behavior. Freud does not often use the term, but when he does, a stimulus is something to be satisfied or warded off.

Psychologists and physiologists, however, have generally used the term for the arousing of a sense organ instead of a whole individual. But they do not wholly agree about this. Some accept both meanings. Neal Miller asserts that "any stimulus has some drive value" (Miller & Dollard, 1941, p. 59). However, Skinner believes that "a drive is not a stimulus," and that although "the term has the unfortunate connotation of a goal or spur to action," we must not be misled by this popular meaning of the word (1938, p. 375). Here, then, is a first area of disagreement in our way of conceiving the stimulus: *does a stimulus motivate the individual or does it merely trigger a response?*

II. Pavlov said that "a stimulus appears to be connected with a given response as cause with effect" (1927, p. 10). This is a forthright assertion. Similarly Watson took as the whole aim of psychology the predicting of the response, given the stimulus, and the specifying of the stimulus, given the response (1924, p. 10). But contrast this with the caution of Hilgard and Marquis. "We refer to a stimulus as an instigator [and] no more is intended than that the stimulus is in some sense the occasion for the response" (1940, p. 73). Evidently what Pavlov and Watson meant by a stimulus is not what Hilgard and Marquis meant. Nearly all psychologists now follow the second line. It is allowed that a stimulus may cause a reflex, but not an act. Woodworth was one of the first to emphasize that the stimulus does not in itself determine the response; factors in the organism intervene to help determine it. The discussion of intervening variables or mediating processes has by now filled volumes.

The same rule is taken to hold for experience. It is allowed that a stimulus may cause a sensation, but not a perception. M. D. Vernon, for example, states that "the nature of the percept is not . . . determined by the physical qualities of the stimulus, but is largely a function of constructive tendencies in the individual" (1952, p. 47). But I have been arguing the opposite for some time, that the percept is in very good correspondence with the physical variables of the stimulus. *Can a stimulus be taken as the sufficient cause of a response, or can it not?* This is a second area of confusion in our concept of the stimulus.

III. Skinner has recently noted that "we frequently define the stimulus by the very doubtful property of its ability to elicit the response in question, rather than by any independent property of the stimulus itself" (1959, p. 355). He suggests no remedy, however, for this doubtful scientific behavior, and he seems to be confessing a sin without pointing the way to salvation. In truth many psychologists do give a circular definition of the stimulus. Skinner himself believed in his first book that "neither term [stimulus or response] can be defined as to its essential properties without the other" (1938, p. 9). Neal Miller has said "a response is any activity by or within the individual which can become functionally connected with an antecedent event through learning; a stimulus is any event to which a response can become so connected" (Miller & Dollard, 1941, p. 59). Miller, in fact, has argued that this circular definition of the stimulus is not only necessary but is theoretically desirable (Koch, 1959, p. 239). He seems to have abandoned completely the specifying of a stimulus by variables of physical energy. But listen to Estes. "By *stimulus,* I refer to environmental conditions, describable in physical terms without reference to the behavior of an organism" (Koch, 1959, p. 455), and Hayek says, "the distinction between different stimuli must be independent of the different effects they have on the organism" (1952, p. 9).

Here is a disagreement. The student of psychophysics will argue that we must define our stimulus

by certain operations of physical science, not by the judgments of our subject. Otherwise how are we ever to discover what stimuli can be discriminated and what cannot? When the stimulus is difficult to specify in objective physical terms, however, investigators tend to avoid the difficulty and describe it as that which is responded to, or that which is perceived. A few go further and, by arguing that an experimenter cannot define the stimulus anyway except in terms of *his* perception, reach a philosophical position of subjectivism. There is an ancient puzzle to which students of philosophy are treated —whether there exists any sound when a tree crashes in the forest with no living being there to hear it. It is a question of how to conceive the auditory stimulus. It seems to remain a puzzle for a good many psychologists.

I think the central question is the following. Is a stimulus that which *does* activate a sense organ or that which *can* activate a sense organ? Some writers imply that a stimulus not currently exciting receptors is not a stimulus at all. Others imply that a stimulus need not excite receptors to be called such. They allow of *potential* stimuli. Witness Guthrie's assertion that stimuli are "potential occasions" for the initiation of sensory activity, and that "the physical stimuli, though present, may not be effective" (Koch, 1959, p. 178). The former conception allows physical energy to be called a stimulus only when some response can be observed; the latter allows of the possibility that stimulus energy may be present without necessarily being responded to. The latter seems the better concept. With the former meaning, one could never speak of a subthreshold stimulus, and this is a useful term. An effective stimulus on one occasion may be ineffective on another. And there are various response criteria by which a threshold can be measured.

The distinction between effective and potential stimuli is made by a few theorists, but its implications have not been traced, and the idea remains undeveloped. The concept of a permanent environment of *objects* is widely accepted, but not the concept of a permanent environment of *potential stimuli.*

The third area of disagreement is this: *must a stimulus be defined independently of the response it produces—in physical terms rather than terms of behavior or sensory process?*

IV. For Pavlov a stimulus could be anything in the terrestrial world. Any event he could think of to use in an experiment he would call a stimulus, and he employed tones, bells, the sound of bubbling water, lights, rotating objects, pictures on a screen, acid in the mouth, food, a scratch on the back, or electric shock. This common sense usage of the term persists among a good many behaviorists. Spence has said that the term stimulus means to him, "the physical or world situation, with its different aspects or features" (1956, p. 39). For Neal Miller anything that is discriminable is a stimulus or, as he calls it, a cue, these terms having the same meaning. For Skinner, a stimulus is simply "a part, or modification of a part, of the environment" (1938, p. 235). To be sure, he says, it must "refer to a class of events the members of which possess some property in common" (p. 34). Because stimuli have this "generic nature," the practice of calling a bell an auditory stimulus and a book a visual stimulus is, as he puts it, "frequently successful" (p. 235). All these writers persist in believing that somehow the things of the environment can *stimulate* us, and they refuse to be worried by the paradox that only receptors at the skin of an individual can actually be stimulated.

This definition of the stimulus is considered naive by perception psychologists. Stimuli are energies, not objects. In Troland's words, "the stimulus may be defined as the specific physical force, energy, or agency which brings about the stimulation of the given receptor system" (1930, p. 9). This conception has the authority of a century's research on the senses. In 1834, Johannes Müller argued that a stimulus was whatever excited one of the "nerves of sense." To the modern neurophysiologist, a stimulus is energy that depolarizes a living cell—especially, but not exclusively, a nerve cell. For Jennings in 1906, studying the ameba, a stimulus was a type of change in the immediate environment that produced a change in behavior (1906, p. 19) and there existed precisely five types: chemical, mechanical, thermal, photic, or electrical. Woodworth says that "a stimulus is any form of energy acting upon a sense organ and arousing some activity of the organism" (1929, p. 223). Koffka wishes to call stimuli "the causes of the excitations of our sense organs" (1935, p. 79), but he, more than any other theorist, faced up to the contradictory meanings of the term and proposed a formal distinction between the "proximal" stimulus and the "distal"

or "distant" stimulus. He made us consider the paradox that although perception and behavior seem to be determined by the distal object, they can in fact only be aroused by the proximal stimulus.

Not all psychologists are willing to grapple with this paradox and, in truth, it is baffling. If the proximal stimulus for a given object is altered with every change of the observer's position in space, if it is different on different occasions, we are faced with an absurdity. We must suppose that a countless family of different stimuli can all arouse the same percept. Most behaviorists speak of the stimulus-object as if, by hyphenating two words with different meanings, the absurdity were removed. As men of common sense they see the need of reducing to one the countless number of stimuli that can arouse a single percept, and in this surely they have a point. But perceptionists, being unable to take this easy way out, struggle to construct theories of how different stimuli might arouse the same percept, the theories of perceptual constancy. So far, no theory has been agreed on. Is it possible that common sense is right without knowing it, and that every family of proximal stimuli arising from one object *is*, in a sense, one stimulus?

Here is a fourth disagreement: *do stimuli exist in the environment or only at receptors?* There is a suggestion that both usages of the term are somehow correct, but it has not been explained.

V. Osgood says that "a stimulus may be defined as that form of physical energy that activates a receptor" (1953, p. 12). But he does not tell us whether he means by a receptor a single cell or a mosaic of receptor cells, that is, a sense organ. Others besides Osgood are undecided about this question, or have not thought about it. Hull knew what he thought. For him, the retinal image was a pattern of stimuli (1943, p. 37) and a single light ray was a stimulus (p. 33). "A stimulus element is a stimulus energy which activates a single receptor-organ" (p. 349). This is straightforward. Woodworth says that "of course the light entering the eye and striking many rods and cones is a collection of stimuli rather than a single stimulus," but in the next paragraph he suggests that "the sudden cessation of a light" is a stimulus (1929, p. 28). Köhler was fairly explicit on the question, saying that an organism responds to "an objective constellation of millions of stimuli" (1929, p. 179) and Koffka also assumed that stimuli on the retina or the skin were local events (1935). But Nissen, on the other hand, asserts that "a stimulus involves a pattern of stimulation, spatial or temporal" (Stevens, 1951, p. 374). Many other writers define stimuli as the occasions for activation of a sense organ, not of a receptor cell, and speak as if a pattern were a stimulus. There is a vast difference between a pattern of stimuli and a stimulus pattern, but we have not sufficiently thought about it. Is a "pattern" a single stimulus or is it a number of separate stimuli?

The notion that a stimulus is what excites a cell, and is therefore *punctate*, seems to many theorists the only rigorous definition. On this account Hull had to introduce the postulate of afferent neural interaction to explain molar behavior as distinguished from molecular responses. The gestalt psychologists had to develop the theory of sensory organization in order to explain perception. But Lashley once said that

> the stimulus to any reaction above the level of a spinal reflex involves not the excitation of certain definite sensory cells but the excitation of *any* cells of a system in certain ratios, and the response may be given to the ratio even though the particular cells involved have not previously been excited in the same way (Murchison, 1934, p. 476).

This passage suggests the idea that higher levels of reaction require us to define higher orders of stimulation. Lashley seems to be saying that a ratio may be itself a stimulus, not just a relation between two stimuli. But note that the gestalt theorists, by conceiving all stimuli as local events, did not come to think in this way.

A controversy has long been going on over the question of how an individual could respond to a relation. It began with Köhler's evidence that a chick will select the brighter of two gray papers instead of the absolute brightness of a particular paper. Köhler thought it demonstrated a relational process in the brain; Spence has gone to great lengths to show that it could be explained in terms of absolute responses to each piece of paper, subject to the so-called principle of stimulus generalization. But the simplest explanation would be that the effective stimulus in the experiment was the direction of the difference in brightness in the field of view. In line with this solution to the problem, students of vision conceive that a margin is a visual stimulus, perhaps *the* visual stimulus, and a margin in the array of light to an eye is strictly a ratio, that is, a relation between measured intensities.

Here is a fifth source of confusion: ***when is a pattern or relation to be considered a single stimulus and when a number of separate stimuli?***

VI. The notion that a stimulus can only be something punctate is related to the notion that a stimulus can only be something *momentary*. The gestalt psychologists pointed out that a melody is perceived, but they never suggested that a melody was a stimulus. The notes of the melody were taken to be the stimuli. But what about the transitions between notes, or the "transients" of acoustical engineering? Are they stimuli? The investigators of speech sounds seem to think so, but the auditory literature of sensation is vague on this question. And if a short transition is a stimulus, why not a long transition or temporal pattern?

In vision, experimenters have not been able to make up their minds as to whether an optical motion was a stimulus or a series of stimuli. The retina and also the skin are very sensitive to motion. It ought to be simple, but the facts of the stroboscope and the phi-phenomenon have been interpreted to imply that it is complex. Motion is taken to be change of location, as it is in classical physics, and it is then reasoned that the impression of location must be fundamental to any perception of a change of location.

On the other hand the generalization is frequently met with that a stimulus is *always* a change. This is very confusing, in fact it is one confusion piled on another. I think that writers who make this assertion have in mind the experiments showing that an unchanging stimulus soon ceases to be effective for perception. They are thinking of sensory adaptation. What changes in that case is not the stimulus but the process of excitation. For the retina, the skin, and the olfactory organ, sensory adaptation does occur. For example, the steady application of an image to a human retina, by the method of artificially stabilizing the image, eventuates in a wholly ineffective stimulus. But note that the steady application of focusable light to a human eye does not. This stimulus never becomes wholly ineffective, even with the best voluntary fixation, because of slight movements of the eye itself. This means that retinal stimulation is by no means the same thing as optical stimulation. They are different stages in the chain of events that leads to vision. A "change in stimulation" means something quite different when it is produced by some adjustment of the sense organ itself than when it is produced by an external event.

Is optical motion, then, meaning a change in the pattern of focusable light to the eye, to be considered a stimulus? Experiments based on this assumption are beginning to appear. In the recent Cornell research with optical transformation (Gibson & Gibson, 1957) we not only think of this as a stimulus, we have come to think of nonchange of pattern as simply a special case. Stability, after all, is only definable as absence of motion. Similarly, a form is definable as a nontransformation. In this conception, sequence is a dimension of stimulation whether or not change occurs.

The great virtue of this conception of sequence is that it suggests a simple solution to the puzzle of perceptual constancy. Two types of nonchange are distinguishable, first, nonmotion of a pattern and, second, invariance of a pattern during motion. The invariant contained in a family of the perspectives arising from a single object is a single stimulus. Hence there is only one stimulus for a single object, and the common sense opinion is right after all.

The sixth conceptual issue is this: ***when does a sequence constitute a single and when a number of separate stimuli; also, can a single enduring stimulus exist throughout a changing sequence?***

VII. Users of the Rorschach test assume that a stimulus field can be either structured or, as they put it, *unstructured*. I could find no explicit definition of unstructured stimulation in the literature but only examples of the material to which the term is applied—inkblots and other items used in the so-called projective tests. The idea of structured stimulation comes from gestalt theory but only from a vague, tentative, and undeveloped hypothesis of gestalt theory—the external forces of organization as distinguished from the internal forces of organization. Koffka, for example, was so preoccupied with the ways in which the individual *structured* his stimulus field that he scarcely considered the ways in which it might already *have* structure (1935). In fact, he wrote sometimes as if it had none, as if all structure had to be imposed on it, because the stimuli themselves were meaningless points.

This uncertainty about the existence of structure in the stimulus for perceived form still persists. But since Koffka's time, and partly inspired by him, some experimenters are beginning simply to

assume it, and to apply mathematics to the structure of a stimulus. They would not agree that an inkblot is in any sense an unstructured stimulus. A picture has one structure, an inkblot has another, but it does not lack structure. That can be said only of a film-color or the cloudless blue sky. The structure of an array may have ambiguous or equivocal components, as Koffka showed, but that is not the same thing. The capacity of light to carry structure to an eye may be impoverished or reduced experimentally but it remains. The structure of light may not specify anything familiar to the subject, or to any observer, but it is a geometrical fact. The subject may be unable to register the structure because it is nonsense to him, or he overlooks it, or he was not told to look for it, or his eyes are defective, or he is too young, or for a dozen other reasons, but it is still in the light. So, at least, some experimenters would argue.

What can be meant by an unstructured stimulus field is thus a matter of disagreement. The seventh question is: *how do we specify the structure of a stimulus?*

VIII. The conception of stimuli as physical energies seems to imply that, in themselves, they have no significance or meaning. Especially if they are considered to be only spots of energy at brief moments of time it is clear that they specify little or nothing about the environment. Light, heat, mechanical, acoustical, chemical, and electrical energy are far from being objects, places, events, people, words, and symbols, but nevertheless they are the only stimuli that can affect receptors. This theory of the meaningless stimulus has been an accepted doctrine for a long, long time in the study of the senses. It leads to the notion of the sense datum—the bare sensation, or raw sensory impression, and thence to the persistent problem of how animals and men can be supposed to perceive objects, places, events, and one another.

Students of behavior, however, without questioning the doctrine of the empty stimulus, often act as if they did not believe it. Beach speaks for comparative psychologists when he says, in describing how birds feed their offspring, "young birds exhibit a gaping response which *stimulates* the parent to place food in the nestling's mouth" (Stevens, 1951, p. 415). He takes it for granted that light rays can specify the event called gaping and refuses to worry about it further. Students of perception do worry about this question, but they are not consistent. On the one hand, they firmly assert that nothing gets into the eye but light of variable wave length and intensity, not objects, or events, or facts of the environment. On the other hand, they often say that light "carries" information about the environment, or that stimuli "provide" information to the perceiver. If this is so, the stimuli must specify something beyond themselves, and they cannot be empty of meaning.

A sort of compromise between the informative stimulus and the empty stimulus is provided by the use of the term *cue*. According to Woodworth, "a cue, as used in psychology, is a stimulus which serves as a sign or signal of something else, the connection having previously been learned" (1958, p. 60). Stimuli are conceived by analogy with messages, or communication in code. Brunswik thought of stimuli as *indicators* of environmental facts, by analogy with pointer readings, emphasizing, however, that they had only a probable connection with the fact in question (1956). Boring has suggested that stimuli may be taken as *clues*, and this term points to Helmholtz's theory of unconscious rational inference from the sense data (Harper & Boring, 1948).

Merely to call the stimulus a cue, sign, signal, message, indicator, or clue does not tell us what we need to know. The question is to what extent does the stimulus specify its source, and how does it do so? Is it possible that the use of these verbal metaphors only prevents us from facing the problem? Or consider the use by modern information theorists of a neutral term like *input*. When they compare the organism to a communication system or to a black box, the internal working of which has to be discovered, are they avoiding the obligation to consider the environment of an organism and the relation of stimuli to the environment?

The problem of the connection between stimuli and their natural sources has not been taken seriously by psychologists. Stimuli have not even been classified from this point of view, but only with respect to the sense organs and the types of energy which carry stimuli. It is a problem of ecology, as Brunswik realized when he wrote about the "ecological validity" of cues (1956). I think the problem has been obscured, and our recognition of it delayed, by our failure to separate it into parts. The connection between natural stimuli and their sources is not the same as the connection between

social stimuli and their sources, for example, the connection between words and their referents. This latter problem, surely, is distinct. Semantics is one thing, ecology is another; and a science of environmental stimuli may not prove to be as difficult as a science of symbols, once we put our minds to it.

I have maintained that optical stimuli, for example, gradients of texture in the light to an eye, specify environmental objects by the relation of *projection*. To me this is not at all the same as the relation by which words specify objects, which I would call one of *coding*. But however this may be, we face another unanswered question, the eighth: *do stimuli carry information about their sources in the world, and how do they specify them?*

SOME POSITIVE HYPOTHESES

Can anything useful be salvaged from these various contradictory usages and definitions? No one could be blamed for being pessimistic about it. S. S. Stevens, who has thought hard and long about stimuli, concluded that it is futile even to attempt a general definition of the stimulus in psychology. Psychology as a whole, he says, can be equated with the problem of defining the stimulus, that is, giving a complete definition of the stimulus for a given response. To be able to do so would require that we specify "all the transformations of the environment, both external and internal, that leave the response invariant." And "for no response have we yet given a complete definition of the stimulus" in this sense (Stevens, 1951, pp. 31f.). If I understand him, what Stevens chiefly had in mind is the puzzle of constancy. He was saying that we do not know how to specify, in the chaos of literal proximal-energy stimulation, the actual cause of a given response. This is a discouraging truth.

But, unlike Stevens, I have hopes, and even some positive hypotheses to suggest. Once the contradictory assumptions about stimulation are made explicit, we can try to resolve them. For one thing we might search for an invariant component in the bewildering variety of functionally equivalent stimuli. Perhaps there is an invariant stimulus for the invariant response, after all. Many sorts of higher order variables of energy may exist, only awaiting mathematical description. They will have to be described in appropriate terms, of course, not as simple functions of frequency and amount. We must not confuse a stimulus with the elements used for its analysis. We must learn to conceive an array not as a mosaic of stimuli but as a hierarchy of forms within forms, and a flux not as a chain of stimuli but as a hierarchy of sequences within longer sequences.

Molar Stimuli. Ever since Tolman, behavior theorists have been agreeing that psychology is concerned with molar responses, not molecular ones. Accordingly we try to observe and measure what an organism is doing, not how all its muscles are contracting. With this kind of observation on the response side there should be a corresponding kind of observation on the stimulus side. We should try to discover what an organism is responding *to*, not what excites all the little receptors. Of course all the muscles may be contracting and all the receptors may be excited, but observation at that level is the job of the physiologists.

The same recommendation can be made for the study of perception. The gestalt theorists have demonstrated the fact of molar experience, but they did not look for molar stimuli. These may very well exist outside the laboratory and, with ingenuity, can perhaps be isolated in the laboratory. If so, we shall have a new and powerful kind of psychophysics.

This conception of molar stimuli is not wholly new. Forty-five years ago, E. B. Holt was convinced that cognition, along with behavior, was a constant function of stimulation. In this he agreed with Pavlov and Watson. But Holt emphasized that the stimulus *of which* cognitive behavior was a function was more abstract and more comprehensive than the stimulus of classical psychophysics. As one passes from reflexes to behavior, the effective stimulus "recedes," as Holt put it (1915, *passim*). By the *recession* of the stimulus he meant that it seems to be located far out in the environment rather than close by in the receptors. And he also meant that as cognition develops, the stimulus of which it is a function recedes more and more. Following this suggestion, one might conclude that a change in response implies a change in the stimulus to which the response is made. Learning would then involve not only an alteration of behavior but also an alteration in the effective stimulus. Presumably its molar character has gone up a stage in the hierarchy.

Potential Stimuli. Evidently the hypothesis of potential stimulation, accepted casually by some theorists, has quite radical but unrecognized impli-

cations. We have long acknowledged the almost unlimited possibilities for new responses in learning theory; why not equally vast possibilities of new stimuli? The environment, so considered, would consist of a sort of reservoir of possible stimuli for both perception and action. Light, heat, sound, odor, gravity, and potential contacts with objects surround the individual. But this sea of energy has variables of pattern and sequence which can be registered by sense organs. They can be explored, either at one station-point or by moving around in the environment. The fields of radiating sound and odor, together with the flux of light rays reflected from surfaces, make it possible to respond to things at a distance. The changes of pattern in time serve as controlling stimuli for locomotion and manipulation. The variables and covariables and invariables of this stimulus environment are inexhaustible.

Surprisingly little has been written about potential stimuli. The sensory physiologists, of course, have read their physics and chemistry. But physical science portrays a sterile world. The variables of physics make uninteresting stimuli. Why is this true? I think it is because psychologists take for stimuli only the variables of physics as they stand in the textbooks. We have simply picked the wrong variables. It is our own fault. After all, physicists are not primarily concerned with stimuli. They have enough to do to study physical energies without worrying about stimulus energies. I think that we will have to develop the needed discipline on a do-it-yourself principle. It might be called ecological physics, with branches in optics, acoustics, dynamics, and biochemistry. We cannot wait for the physical scientists to describe and classify potential stimuli. The variables would seem to them inelegant, the mathematics would have to be improvised, and the job is not to their taste. But it is necessary. And if successful, it will provide a basis for a stimulus-response psychology, which otherwise seems to be sinking in a swamp of intervening variables.

Consider, for example, the physics (that is to say the acoustics) of speech sounds. As recently as 1951, in the *Handbook of Experimental Psychology* (Stevens, p. 869), the fact that a word is perceptually the same when whispered as it is when shouted was taken to prove that the physical characteristics of sound waves, frequency, intensity, and so on, cannot tell us about speech. Speech perception would require a psychological theory, not physical measurement. But the invention of the sound spectrograph seems to have shown that certain higher order variables of acoustic energy are the critical constituents of speech and the stimuli for hearing it. These newly discovered invariant patterns of sound are completely physical, even if they had not previously been studied in physics. What was needed to understand the psychophysics of hearing words was not more psychology but more physics.

For another example consider the optics of an array of light. The physical variables applying to the point source and the image point do not explain the seeing of a surface. But my own work shows that the variables of an optical *texture* do account for the seeing of a surface, and that by manipulating textures an experimenter can produce synthetic perceptions of objects (Gibson, Purdy, & Lawrence, 1955). Gradients, patterns, and other invariants are not part of existing geometrical optics, but they are physical facts. What was needed for a psychophysics of visual perception was not more theorizing about cues but more attention to geometrical optics.

Effective Stimuli. An effective stimulus can now be defined. It is one which arouses receptor activity, or recorded neural impulses, or sense organ adjustments, or overt responses, or verbal judgments —whichever criterion one chooses. Note that the idea of fixed innate thresholds of sensation is rejected. It always was a myth, for every psychophysical experimenter knows that the threshold obtained depends on the method used and the response criterion chosen.

In short, whether or not a potential stimulus becomes effective depends on the individual. It depends on the species to which he belongs, on the anatomy of the sense organs, the stage of maturation, the capacities for sense organ adjustment, the habits of attention, the activity in progress, and the possibilities of educating the attention of the individual. Such facts make up the field of perceptual development and perceptual learning. At the lower levels they are called facts of sensory physiology; at the higher levels, facts of attention or exploration, but they are all one problem. Animals seem to be driven to make potential stimuli effective. They use their receptor equipment, probably, in as great a variety of ways as they use their motor equipment. From this point of view, it seems to me, the senses begin to make sense.

Stages of Specificity. Johannes Müller began the study of the way in which the modes of experience are specific to the excitations of nerve fibers. Sherrington and others showed how the excitations of fibers were generally specific to the patterns of the stimulus. Ecological physics will tell us the extent to which the proximal stimuli are specific to their sources in the world. If experience is specific to excitation, and excitation to stimulation, and stimulation to the external environment, then experience will be specific to the environment, within the limits of this chain of specificities. The first two stages have long been under investigation. The last is ripe for study. There has been a controversy over whether or not visual stimuli can specify their objects (for example, Cantril, 1950), but it can be settled, for the facts are discoverable, and arguments should await evidence.

The Informative Capacity of Molar Stimuli. If the structure and sequence of stimulus energy can be analyzed, potential stimuli can be described and arranged in a hierarchy. There will be subordinate stimuli and superordinate stimuli, of lower order and higher order. So conceived it is reasonable to assume that stimuli *carry information* about the terrestrial environment. That is, they specify things about objects, places, events, animals, people, and the actions of people. The rules by which they do so are to be determined, but there is at least enough evidence to warrant discarding the opposite assumption under which we have been operating for centuries—that stimuli are necessarily and intrinsically meaningless.

Natural Stimuli, Pictorial Stimuli, and Coded Stimuli. I have suggested that, instead of continuing to employ the careless analogies of our present loose terminology for stimuli—cues, clues, signals, signs, indicators, messages, inputs, and the like—we make a systematic study of the laws by which stimuli specify their sources. We need to know the laws of stimulus information. Almost certainly these will not be the laws which govern the transmission of information in human systems of communication. The natural world does not literally *communicate* with the sense organs. The potential physical stimuli arising from an event are not to be compared to the physical stimulus arising from the *word* for that event. We cannot hope to understand natural stimuli by analogy with socially coded stimuli, for that would be like putting the cart before the horse. Just this, however, is what we tend to do when we speak of the "signs" for depth perception and the "messages" of the senses. We cannot afford to speak of coded information for the sense organs when we mean stimuli, for some of these are coded and some are not.

A systematic study of the specifying power of stimuli will put the problem of meaning in perception on a new footing. It will take several forms, depending on the kinds of relations discovered. My guess is that there will be at least three, corresponding to the stimuli from things, from pictures, and from words. It is true that men, besides learning to perceive objects, also learn to apprehend things by way of perceiving pictures and words. These mediated perceptions get mixed with direct perceptions in the adult. But we shall have to disentangle them before we can have a complete theory of human perception.

CONCLUSION

The foregoing distinctions and assumptions seem promising to me. But I would agree that a stimulus theory cannot be established by merely asserting it. The scientific question is whether all these new kinds of stimuli exist. I suggest that we look for them in the environment and then try to bring them into the laboratory.

It is still true that the stimulus is the prime independent variable of a psychological experiment. I quote from Underwood (1957):

> One may vary more than one stimulus condition in a given experiment . . . but to draw a conclusion about the influence of any given variable, that variable must have been systematically manipulated alone somewhere in the design. Nothing in analysis of variance, covariance, Latin squares, Greco-Latin squares, or Greco-Arabic-Latin squares has abrogated this basic principle (p. 35).

If Underwood is right, the secret of a good experiment is to discover the relevant stimulus before doing the experiment. The moral of my argument is that a systematic search for relevant stimuli, molar stimuli, potential stimuli, invariant stimuli, specifying stimuli, and informative stimuli will yield experiments with positive results. Perhaps the reservoir of stimuli that I have pictured is full of elegant independent variables, their simplicity obscured by physical complexity, only waiting to be discovered.

REFERENCES

BRUNSWIK, E. *Perception and the representative design of experiments.* Berkeley: Univer. California Press, 1956.

CANTRIL, H. *The "why" of man's experience*. New York: Macmillan, 1950.

FREUD, S. *Collected papers*. London: Hogarth Press, 1949.

GIBSON, J. J. & GIBSON, E. J. Continuous perspective transformations and the perception of rigid motion. *J. exp. Psychol.*, 1957, **54**, 129–138.

GIBSON, J. J., PURDY, J., & LAWRENCE, L. A method of controlling stimulation for the study of space perception: The optical tunnel. *J. exp. Psychol.*, 1955, **50**, 1–14.

HARPER, R. S., & BORING, E. G. Cues. *Amer. J. Psychol.*, 1948, **61**, 343–351.

HAYEK, F. A. *The sensory order*. Chicago: Univer. Chicago Press, 1952.

HILGARD, E. R., & MARQUIS, D. G. *Conditioning and learning*. New York: Appleton-Century-Crofts, 1940.

HOLT, E. B. *The Freudian wish*. New York: Holt, 1915.

HULL, C. L. *Principles of behavior*. New York: Appleton-Century-Crofts, 1943.

JENNINGS, H. S. *Behavior of the lower organisms*. New York: Columbia Univer. Press, 1906.

KOCH, S. (Ed.) *Psychology: A study of a science*. Vol. 2. New York: McGraw-Hill, 1959.

KOFFKA, K. *Principles of gestalt psychology*. New York: Harcourt, Brace, 1935.

KÖHLER, W. *Gestalt psychology*. New York: Liveright, 1929.

MILLER, N. E., & DOLLARD, J. *Social learning and imitation*. New Haven: Yale Univer. Press, 1941.

MURCHISON, C. *Handbook of general experimental psychology*. Worcester: Clark Univer. Press, 1934.

OSGOOD, C. E. *Method and theory in experimental psychology*. New York: Oxford Univer. Press, 1953.

PAVLOV, I. P. *Conditioned reflexes*. (Trans. by G. V. Anrep) London: Oxford Univer. Press, 1927.

SKINNER, B. F. *The behavior of organisms*. New York: Appleton-Century-Crofts, 1938.

SKINNER, B. F. *Cumulative record*. New York: Appleton-Century-Crofts, 1959.

SPENCE, K. W. *Behavior theory and conditioning*. New Haven: Yale Univer. Press, 1956.

STEVENS, S. S. (Ed.) *Handbook of experimental psychology*. New York: Wiley, 1951.

TROLAND, L. T. *Psychophysiology*. Vol. 2. New York: Van Nostrand, 1930.

UNDERWOOD, B. *Psychological research*. New York: Appleton-Century-Crofts, 1957.

VERNON, M. D. *A further study of visual perception*. Cambridge: Cambridge Univer. Press, 1952.

WATSON, J. B. *Psychology from the standpoint of a behaviorist*. Philadelphia: Lippincott, 1924.

WOODWORTH, R. S. *Psychology*. New York: Holt, 1929.

WOODWORTH, R. S. *Dynamics of behavior*. New York: Holt, 1958.

TOWARD A COMPARATIVE PSYCHOLOGY OF LEARNING[1]

M. E. BITTERMAN

Bryn Mawr College

IN the early years of this century, when the experimental study of animal intelligence was just getting under way, many different species were brought into the laboratory. From the very beginning, to be sure, there was some preference for the higher animals, but, on the whole, early interest ranged rather widely up and down the phylogenetic scale. Before long, however, the scope of research narrowed. Attention became fixed on a small number of mammalian forms, which were chosen primarily for reasons of convenience, and treated as representative of animals in general, with the cheap and docile rat easily leading all the rest.

A set of curves which nicely illustrates this trend was published some years ago by Beach (1950). Based on a count of papers appearing between 1911 and 1948 in the *Journal of Comparative and Physiological Psychology* and its forerunners, the *Journal of Comparative Psychology* and the *Journal of Animal Behavior*, the curves show how interest in the rat mounted rapidly, while interest in the submammalian forms declined. In the thirties, a stable pattern emerged, about 60% of the papers dealing with the rat, about 30% with mammals other than the rat, and about 10% with the lower forms (the submammalian vertebrates and the invertebrates). If we make the computations required to bring these curves up to date, we find no significant change in the state of affairs decried by Beach a decade ago. Please note that Beach's curves are based on all papers published in a single journal. If we count only the papers on learning, and look at a broader sample of journals, the effect becomes even more striking. About 90% of our work on animal learning has been done with the rat.

Of course, specialization has its advantages, and, if the process of learning were essentially the same in all animals, it would be rather improvident of us to diffuse our efforts over the phylogenetic scale; but the only way to find out whether the process of learning *is* essentially the same in all animals is to make some careful studies at widely separated points in the scale. We have not made such studies. It is our willingness to *assume* that the process of learning is essentially the same in all animals which has been responsible in large measure for our concentration on the rat.

The assumption comes to us from Darwin, who was interested in psychological as well as in structural continuity. For Darwin, novelty was incompatible with continuity, and his purpose was to demonstrate that any seemingly unique property which made its appearance in the animal series really was not unique at all—that some hint or promise of it always could be discovered at an earlier point in the series. On the basis of the only evidence available to him—the tall tales of naturalists and zookeepers—Darwin decided that phylogenetic differences in intelligence were differences "of degree and not of kind." Not even in the intelligence of man could Darwin find anything new. Capacities which *seemed* peculiar to man, he explained, could be traced to man's use of language, which itself—"half-art and half-instinct"—clearly bore "the stamp of its gradual evolution" (1871, pp. 105–106). Darwin's formula for bridging the gap between man and the infrahuman animals has a contemporary flavor; it finds wide use even today.

The early comparative psychologists were not entirely convinced by Darwin's arguments. Critical of his anecdotal evidence, and proud of their own new methods, they were determined to have a look for themselves; but their skepticism did not last long. Observing a variety of animals in problem boxes, mazes, and discriminative situations of one sort or another, they were more impressed by the similarities in behavior than by the differences, and quickly succumbed to the Darwinian view. Thorndike, himself, showed the way. At the outset, his contempt for certain features of Darwin's position was undisguised—"man was no more an animal with language," he wrote, "than an elephant was a

[1] This paper is adapted from an address to the Eastern Psychological Association at its 1959 meeting. The research described has been supported by Grant M-2857 from the National Institute of Mental Health and Contract Nonr-2829(00) with the Office of Naval Research. Reproduction of this paper in whole or in part is permitted for the purposes of the United States Government.

cow with a proboscis" (1898, p. 87)—but, prepared though he was to find important phylogenetic differences in mode of learning, he found none. His very earliest observations led him to the "working hypothesis" that the process of learning was essentially the same throughout the phylogenetic series. All animals, for Thorndike, were "systems of connections subject to change by the law of exercise and effect" (1911, p. 280), differing only in the particular connections formed and in the ease of connection. The influential Watson took the same position, although he rejected the law of effect and tried to get along with exercise alone—"in passing from the unicellular organisms to man," said Watson, "no new principle is needed" (1914, p. 318)—and gradually Thorndike's working hypothesis became transformed into an article of faith. The transformation is reflected in Beach's curves. As a working hypothesis, the proposition that learning is essentially the same in all animals led to the study of many animals. As an article of faith, it led, by the principle of least effort, to concentration on one.

Specific illustrations of the contemporary faith are not hard to find. Dollard and Miller base their account of human personality and psychotherapy on the assumption that, as they put it, "any general phenomena of learning found in rats will also be found in people" (1950, p. 63). They admit that "people may display additional phenomena not found in rats," but at no point in their book can one find any indication that they take the possibility very seriously. What they refer to as "the higher mental processes" of man they explain in terms of mediating responses, primarily linguistic in character, to which the general laws of stimulus-response connection are assumed to apply.

Another interesting example of the contemporary faith was provided by Skinner a few years ago, when, in an address to the Eastern Psychological Association, he attempted to justify his disregard of the standard statistical procedures. Those procedures are necessitated, he said, only by the fact of individual differences, and they lose all point when individual differences are eliminated "in advance of measurement." To demonstrate the extent to which his own method eliminates irrelevant sources of variability, he presented, among others, the curves shown in Figure 1. The curves were made by three animals trained in three different laboratories on a multiple fixed-interval-fixed-ratio schedule of reinforcement—one by a pigeon, one by a rat, and one by a monkey. "Pigeon, rat, monkey, which is which?" Skinner asked, and he answered:

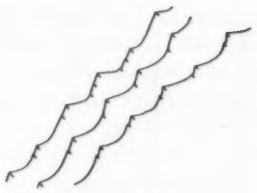

FIG. 1. Cumulative-frequency curves for pigeon, rat, and monkey (not necessarily in that order) trained on a multiple fixed-interval-fixed-ratio schedule of reinforcement. (From Skinner, 1956.)

> It doesn't matter. Of course, these three species have behavioral repertoires which are as different as their anatomies. But once you have allowed for differences in the ways in which they make contact with the environment, and in the ways in which they act upon the environment, what remains of their behavior shows astonishingly similar properties. Mice, cats, dogs, and human children could have added other curves to this figure. And when organisms which differ as widely as this nevertheless show similar properties of behavior, differences between the same species may be viewed more hopefully. Difficult problems of idiosyncrasy or individuality will always arise as products of biological and cultural processes, but is it the very business of the experimental analysis of behavior to devise techniques which reduce their effects except when they are explicitly under investigation (1956, pp. 230–231).

The function of these curves, the context suggests, was not to demonstrate that the process of learning is essentially the same in pigeon, rat, and monkey; that was assumed. The function of the curves was to demonstrate the power of the method, which was found in the fact that it *reveals* the essential similarity of the three animals despite substantial sensory and motor "idiosyncrasies."

Whatever Skinner's purpose, his curves do, of course, show some interesting similarities in the learned behavior of quite different animals, and evidence of the same sort has been accumulating since the turn of the century. A comparable set of curves dating back to the years between 1901 and 1904 is shown in Figure 2. Like those of Skinner, they represent the work of three independent investigators working with three different animals—a monkey, a rat, and a bird (in this case, a sparrow). The apparatus employed was the Hampton Court maze, as popular in its time as Skinner's box is today. "Monkey, rat, sparrow, which is which?" it was asked at the turn of the century, although the question went to the nature of phylo-

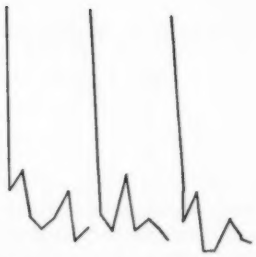

FIG. 2. Relative-error curves for sparrow, rat, and monkey (not necessarily in that order) trained in a Hampton Court maze. (After Small, 1901; Kinnaman, 1902; and Porter, 1904.)

genetic differences rather than to the power of the maze method, and the answer came: "It doesn't matter." That answer is no more warranted now than it was 50 or 60 years ago. Attractive as the Darwinian hypothesis may seem, it should not be accepted on the basis of superficial resemblances of this sort. The resemblances may, in fact, be more than superficial, but we shall never know until our level of inquiry becomes more than superficial.

Our skepticism must, of course, be based to a considerable extent on some guesswork about the significance of phylogenetic differences in brain development. Thorndike, too, was interested in brain development, but he was inclined to attribute to it a purely quantitative significance; he early speculated that brain development meant merely the capacity for a greater number of connections and greater efficiency of connection. Even in recent years this kind of thinking has been common—for example, Miller (1951) has suggested that brain development may have no more significance than the addition of banks of keys to an automatic calculator—but brain development is more than an increase in the mass of brain tissue. New structures, such as the cortex, have appeared, which suggest the possibility of new functions. It is interesting to note, by the way, that the acceptance of the Darwinian hypothesis was accompanied by a growing disregard for the facts of brain structure. After giving up their inquiry into animal consciousness, the early comparative psychologists were at some pains to establish that there still remained to them an area of investigation different from that of the zoologists, and that their methods and findings were as important as those of the zoologists. If some primitive experiments with a maze or a problem box suggested that the process of learning was the same in monkey and sparrow, why then perhaps the difference in brain development had no fundamental significance. That was the attitude which Watson expressed when, speaking of man's frontal lobes, he said that "simply because we have the tissue" it does not necessarily "have a life and death significance" (1914, p. 320). To be sure, we must agree with Watson in principle. If extensive and detailed investigation at a number of widely separated points in the phylogenetic scale reveals no fundamental difference in the mode of learning, we must sooner or later conclude that differences in brain structure have no relevance for mode of learning; but we should not be quite as easily persuaded as Watson.

Of course, not everyone *has* been as easily persuaded. From time to time, skeptical voices have decried the facile acceptance of Darwin's hypothesis and have urged the development of a systematic comparative psychology, but they have not often been able to propose a reasonable way of going about it. One of the most respected advocates of comparative research was Yerkes (1917), whose approach was that of the mental tester. Yerkes' idea was to develop a standardized situation appropriate to the sensory-motor capacities of a variety of species, and to order the species on the basis of their performance in it. This seems to be the common conception of comparative psychology even today. For example, following the lead of Harlow (1949), a number of investigators recently have been comparing raccoons, children, chimpanzees, and monkeys of varying description in terms of their rates of improvement in long series of discriminative problems. No wonder there has been little enthusiasm for comparative psychology so conceived. When I mention to someone that I am making comparative studies of learning, his first question is apt to be, not "How are you going about it?" but "How do you know that your measures are comparable?" He assumes automatically that I am comparing the animals in terms of certain absolute measures of performance from which I propose to infer differences in ability, and he wonders, then, how I can ever be sure that differences in performance *are* due to differences in ability rather than to sensory, or to motor, or to motivational differences. I sometimes find difficulty in convincing such a person that there is another kind of comparative research.

Suppose that I introduce a target into the living tank of a fish and reward the animal with a pellet of food for pushing against it.[2] The measure of performance is latency of response—the number of seconds which elapse between the introduction of the target and the animal's response to it—and I plot a curve showing change in latency over a series of reinforced trials. After the latency of response has fallen to a stable level, I terminate reinforcement and plot the progressive increase in latency which ensues. This procedure is directly analogous to one which has been used rather widely with the rat, and the results obtained—the acquisition and extinction curves—are quite similar to those for the rat. The absolute latencies may be very different in the two animals, but the *relation* between latency and trials is very much the same under conditions of consistent reinforcement and nonreinforcement, and it is the relation in which I am interested. In the past 50 or 60 years, we have begun to work out many such relations for the rat, and our theory of learning in the rat represents an effort to find order and meaning in those relations. I am interested now in the extent to which similar relations are to be found for the fish, because I am interested ultimately in determining whether a single theory will fit both animals.

Here, then, is the plan: Taking the much-studied rat as a point of departure, I select for comparison another animal—a fish—which is different enough from the rat to provide a marked phylogenetic contrast, yet similar enough to be studied under analogous conditions. The two animals are not to be compared in terms of their absolute scores in some standard apparatus. Work with the fish, like work with the rat, is to be directed at the discovery of functional relations. Its goal is a theory of the fish with which to compare the theory of the rat (Bitterman, Wodinsky, & Candland, 1958).

This conception of the comparative psychology of learning (as an attempt to determine whether learning in different animals may be understood in terms of the same set of laws or whether different laws are required) is not a new one. It was implied in the writings of Thorndike, and in the work of a small number of subsequent investigators, such as Schneirla (1946), who continued to take the comparative problem seriously. It was stated quite clearly by Hull (1945), although he himself did not give much weight to the possibility that different laws might operate at the different phylogenetic levels. As yet, however, there has been no explicit attempt to deal with a methodological difficulty which has seemed to stand between this conception and fruitful research—a difficulty similar to that by which the mental-test strategy is utterly defeated.

To the extent that functional relations of the same kind appear in fish and rat no one is troubled. The principle of parsimony leads us all to assume—though perhaps incorrectly—that the same process of learning is operating in the two forms. Suppose, however, that different functional relations appear. We may not be as ready then to infer that the underlying processes of learning are different. We may wonder, and with good reason, whether some peripheral factor—sensory, or motor, or motivational—is responsible for the discrepancy, and we may despair of ever being able to control such variables in comparative experiments. How should we hope to find a situation whose sensory and motor demands upon our fish are equivalent to those of the runway or the bar pressing apparatus upon the rat? How should we hope to produce in our fish levels of motivation comparable to those which commonly prevail in experiments with the rat? Fortunately, the problem is not an insoluble one. While the prospects for *control by equation* are slim indeed, there is available a perfectly suitable alternative, which we may speak of as *control by systematic variation*.

Consider, for example, the paradoxical relation between consistency of reinforcement and resistance to extinction which has been established in a variety of experiments with the rat, and suppose that a like effect fails to appear in an analogous experiment with fish—that a partially reinforced group of fish extinguishes *more* rapidly than a consistently reinforced group. Are we led at once to the conclusion that different processes of learning operate in fish and rat? Certainly not. We must consider carefully the possibility that the relation between consistency of reinforcement and resistance to extinction is influenced by certain contextual variables, such as drive level or effortfulness of response, and that in our experiment with the fish we have permitted one of these variables to take on a value well beyond the range prevailing in experiments with the rat. Interactive interpretations of this kind can be tested without great difficulty. It does

[2] For methodological details, see Longo and Bitterman (1959).

not matter that we are unable to equate such variables as drive level in fish and rat; we know how to manipulate them and that is enough. If, working with *either* animal, we find the same relation between consistency of reinforcement and resistance to extinction at diverse levels of drive, the possibility that a difference *between* animals may be explained in terms of drive level is ruled out, and other interactive hypotheses can be tested in like manner. It may be well to note that the example which I have chosen is more than hypothetical. The initial resistance to extinction of *Tilapia macrocephala* (the fish with which my colleagues and I have been working) *is* greater after consistent than after partial reinforcement, and the outcome is the same with spaced or with massed trials, after relatively small or relatively large amounts of training, and at different levels of drive (Longo & Bitterman, 1960; Wodinsky & Bitterman, 1959, 1960). My main concern here, however, is with general strategy.

Given the decision to study some simple animal (such as the fish) under conditions analogous to those which have been used for the study of learning in the rat, with functional relations to provide the basis for comparing the two forms, there remains the question of where to begin. Intuitively, there seem to be many points at which the learning of fish and rat might be compared with profit, and my only verbalizable guiding principle has been to begin with conditions whose effects at the level of the rat have resisted analysis in terms of our simplest constructs. Contemporary learning theory seems to me to underestimate the rat, but I have developed a certain sentimental attachment to it, and the idea has occurred to me that a more comfortable place might be found for it at some earlier place in the animal series. That is, the rat phenomena which embarrass the theory simply may not appear in some more primitive form. Consider again the paradoxical effect of partial reinforcement on resistance to extinction in the rat, which once it seemed possible to explain in terms of stimulus generalization. The neo-Guthrians have attempted to bolster the failing generalization principle with a rather dubious habituation principle, but even the two principles together cannot deal adequately with all of the data. Suppose, however, that partially reinforced fish showed the paradoxical effect when and only when they could be expected to do so on the basis of these simple principles. We might then be willing to conclude that contemporary S-R theory is appropriate at the level of the fish, although new processes of learning came into operation at the level of the rat. As it happens, the paradoxical effect does not appear in our fish even where these simple principles suggest that it should; but I am only half serious, of course, about the possibility of finding an exemplary S-R creature in the lower reaches of the phylogenetic scale. I do, however, have considerable confidence in the notion that the mammalian phenomena which confound the theory are less likely to appear in more primitive species than are those which have suggested its basic postulates, and I have chosen to begin with the former because I am interested in maximizing the probability that functional differences will be discovered if they do in fact exist.

If the much-studied rat is to provide a phylogenetic frame of reference for comparative work with other animals, the choice of starting point also must depend on the structure of existing literature on the rat. Despite the volume of that literature, it is not exactly rich in well-defined functional relations, probably because so much of our past effort has been devoted to a search for crucial experiments designed to resolve certain very broad theoretical problems which arose out of the very earliest work on animal intelligence. I certainly do not regret this effort. The problems are real ones, and we have gained much insight into them. It is significant, I think, that nothing much in the way of well-defined functional relations has been forthcoming even from those who have rejected the traditional questions and advocated a vacant empiricism; in the hands of the Skinnerians, for example, batteries of expensive automatic equipment have yielded little more than an idiosyncratic assortment of kymograph tracings scarcely capable of quantitative analysis. Theoretical concern may not lead of inevitable bar. The position is easier to defend that a mature investigator does not take the trouble necessity to the plotting of functional relations, but certainly it must not be thought to constitute an to make systematic measurements unless they promise to clarify some larger problem. In any event, the theoretical concern which I am here attempting to delineate encourages the discovery of functional relations—both in new animals and in old. I must note that my interest in comparative work with new animals has not made it possible for me to give up work with the rat. My original notion—that I

would study only the new animals and compare the functional relations obtained with those already available in the literature for the rat—proved far too simple. Sometimes an experiment with the fish gives rise to questions about the rat for which there are no adequate answers in the literature, and I may be too interested in those answers to be content to wait until somebody else happens to supply them.

Exploratory work with new animals is not for the impatient; the ratio of achievement to effort, at least in the beginning, is rather small, which should not be surprising in the light of the history of research on the rat. One must learn how to keep each new animal in the laboratory, how to motivate it, and something about its perceptual and motor capacities, before the quest for an appropriate set of experimental conditions even can be begun. The difficulties are many, and failure much more common than success.

In our work with the fish, as it happens, my colleagues and I have progressed at a quite satisfactory rate. We have succeeded in developing a set of efficient, objective techniques, well suited to a variety of species, which permit us to attack the problems of learning in fish on a broad front. One of these techniques, which has been mentioned already, and to which we have given most of our attention thus far, involves the presentation of a target at which the animal is trained to strike for food reward. After some preliminary work with a crude, mechanical system (Haralson & Bitterman, 1950), we developed a more sensitive and reliable electronic one (Longo & Bitterman, 1959). The present target is a disk of metal mounted on a light rod which is inserted into the needle holder of a crystal phonograph cartridge, and the amplified output of the cartridge is used to operate a set of relays which record and reward response. A single-target apparatus may be used either in Thorndikian or in Skinnerian fashion. That is, one may measure the latency of response in discrete trials, each trial beginning with the introduction of the target and terminating with its removal; or one may measure rate of response to a continuously available target. With two targets introduced simultaneously, choice may be measured, as in the T maze or jumping apparatus. More recently, we have developed two additional techniques, one for the study of escape and avoidance, which is so closely patterned after Warner's shuttle box as to require no further description here, and one for the study of classical conditioning (Horner, Longo, & Bitterman, 1960a, 1960b). In our classical conditioning situation, the US is brief shock, and a paddle inserted into the water detects the generalized response which the CS soon begins to elicit. Again a phonograph cartridge plays an important role, its amplified and integrated output driving a counter which provides an objective measure of response-magnitude. The scope of comparative research made possible by these three techniques is considerable, but they came, it must be emphasized, only after several years of trial which yielded very little in the way of useful data. Our work with other animals often has progressed at a much less satisfactory rate, and in some cases we have made almost no progress at all.

An invertebrate to which we have devoted a good deal of fruitless effort is the Bermuda land crab, *Gecarcinus lateralis*, which is quite easy to keep in the laboratory. It lives at 78° F. on some moist sand in a small fish tank, and it does nicely on half a peanut, some lettuce, a bit of raw carrot, and piece of eggshell once each week. It even does nicely for months on no food at all, which was a source of some disappointment to us, since we had been led to believe that its appetite was good. Failing at first to elicit consistent interest in food under our experimental conditions, we turned to escape. We tried light and heat which did not prove suitable, and then shock, which proved quite disorienting, often causing the animal to drop most of its limbs. At last, after some months of fruitless effort, we hit upon immersion in distilled water, which (probably because of its interference with salt regulation) seemed to produce a rather sustained effort to escape.

The uppermost drawing in Figure 3 illustrates the earliest form of the apparatus which we then proceeded to develop, an adaptation of a long, narrow fish tank that we happened to have on hand. We painted one end black, the other end white, and made a clear plastic starting compartment for the center, with two yoked guillotine doors that were raised simultaneously to permit choice. A coarse wire mesh ramp at one end led up out of the water to the home cage of the animal, also equipped with a guillotine door. The other end offered no escape from the water, but a dummy cage was set there for visual balance. In this situation, our crabs rapidly developed a preference for the positive side, where-

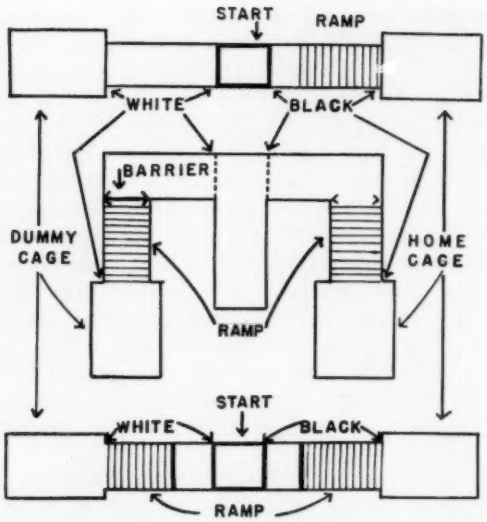

FIG. 3. Apparatus for the study of learning in the crab.

upon we constructed the more elaborate apparatus sketched in the central portion of Figure 3. There was a stem leading to a T-shaped choice-point, and there were ramps on both sides, each of which could be blocked off by a barrier which was not visible from the choice-point. In this apparatus, the crabs did not learn at all; they would remain for long periods in the stem, scrabbling ineffectively at the front wall. When we removed the stem and installed a starting compartment of the earlier kind, performance improved, but the elbows still seemed to make for considerable confusion. At last, we removed the end-sections of the apparatus, returning to the original linear pattern, and the animals regained their earlier efficiency. Several months were lost in making the circle. The new apparatus, sketched in the lower portion of Figure 3, does, of course, have features that the earliest did not. There are two ramps, with a guillotine door before each. Both doors are down to begin with, and the door on the correct side is raised only after the subject has made a correct choice.

Under these conditions, it was possible for us to complete our first formal study of learning in the crab—an experiment on habit reversal (Datta, Milstein, & Bitterman, 1960). In the course of that work, however, we had occasion to become dissatisfied with our technique. Clearly, we were able to achieve considerable control over the behavior of our animals, but the relatively low accuracy of our control group (which was not reversed) suggested the possibility that a higher level of drive should be sought. Now we are using dilute solutions of acetic acid in place of the distilled water. The acid strikingly improves both speed and accuracy of performance, but the animals do not survive many experimental sessions. Perhaps we shall be able to find a concentration strong enough to motivate the animals satisfactorily yet weak enough that it will not impair their health, or perhaps we shall be able to find a buffering procedure to promote post-experimental recovery from the effects of immersion in strong solutions. So the search for a suitable motivating technique continues.

Another arthropod with which we have been working recently is the blowfly, *Phormia regina*. Our interest in this animal was stimulated by the physiological investigations of V. G. Dethier (for example, Dethier & Bodenstein, 1958), who taught us how to breed it and how to keep it in the laboratory. We have studied the fly in a simple runway and in a number of choice-situations, such as that shown in Figure 4. The work is done with harnessed individuals. The subject is anesthetized with CO_2, and a leash of light thread with a loop in its distal end is fixed to the dorsal surface with a bit of wax. The operation does not impair flying ability, but flight is limited in the experimental situation by a length of wire threaded through the loop at the end of the leash. In the apparatus sketched in Figure 4, the wire is curved, permitting the animal to alight only on one or the other of the two targets, one of which is baited with sugar solution. After innumerable variations of method and procedure, our efforts to develop situations suitable for systematic studies of learning in the blowfly seem finally to be meeting with a certain degree of success.

An animal with which we have had no success whatsoever as yet is the earthworm. For a long

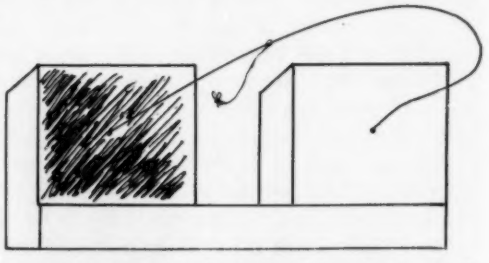

FIG. 4. Apparatus for the study of learning in the fly.

time I have been wanting to make a systematic study of the process of classical conditioning in a really primitive animal, and for the kind of work I have in mind it is not enough merely to look at the animal and try to decide whether or not it twitches when the CS is presented; an objective measure of response-magnitude is required. A few years ago, I came upon a paper by Galambos (1939) which seemed to meet the need. Galambos was interested in the movement of the earthworm, not in its learning, but his work led me to explore the possibility of a Bechterevian apparatus for the worm, the most recent version of which is shown in Figure 5. The animal lies in a narrow, covered trough, in the floor of which two electrodes are set, two loops of linen thread having been sewn previously into the dorsal musculature—one anteriorly and one posteriorly. A thread runs from the posterior loop to a fixed post, and another thread runs from the anterior loop to a spring which is attached to a second post. The spring is weak enough to permit withdrawal in response to shock, but strong enough to encourage return to the basal position when shock terminates. A third thread runs from the anterior loop to a kymograph on which response is recorded, as shown in the tracing. In this situation, brief shock elicits a response that looks very much like the flexion which shock to the limb elicits in the dog, and a neutral stimulus paired with shock may elicit a weak copy of the same response. Usually, however, the CR is a much more subtle affair which, though clearly visible to the naked eye, may fail entirely to activate the recording pen. After a good deal of time and effort devoted to the development of this apparatus, I have decided reluctantly that it should be abandoned in favor of an electronic technique which promises greater sensitivity.

Having emphasized the difficulties and disappointments which seem to be inherent in this exploratory work with relatively unfamiliar animals, I must note that it affords a rather special kind of enjoyment—a kind of enjoyment that I began to taste as an undergraduate in Schneirla's laboratory, but that I found only rarely in my subsequent work, which was more in accord with the spirit of the times. For many years, the field of animal learning has been dominated by a controversial, deductive spirit. Most of us have acted as though we knew all about the learning process, and as though the only purpose of our experiments was to

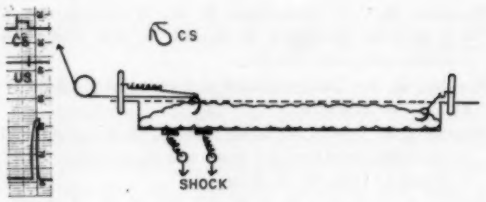

FIG. 5. Apparatus for the study of learning in the earthworm.

demonstrate the validity of our convictions. The controversy was fun, of course, and to some extent even productive—we managed certainly to accumulate a considerable amount of data on learning in the rat—but in my recent dealings with fish, and fly, and crab, and worm, I have come again to a kind of research that is at once more satisfying and more productive. Its function is inquiry, not proof. When I ask about the effects of partial reinforcement on resistance to extinction in the fish, or about the course of habit reversal in the crab, I have not the slightest notion what the answer will be; I can only wait eagerly for the outcome of my experiments. I am not suggesting, of course, that the same attitude cannot be taken in work with the rat. I am suggesting only that it comes more readily in work with the primitive animals, which are so far removed phylogenetically from those which have been taken as models by the various parties to the controversy—the lower mammals, on the one hand, and, on the other, man. Broadening the phylogenetic base of our work will facilitate the broadening of our outlook, and perhaps one day we shall be able to approach even the higher forms in the same spirit of discovery.

REFERENCES

BEACH, F. A. The snark was a boojum. *Amer. Psychologist*, 1950, **5**, 115–124.

BITTERMAN, M. E., WODINSKY, J., & CANDLAND, D. K. Some comparative psychology. *Amer. J. Psychol.*, 1958, **71**, 94–110.

DARWIN, C. *The descent of man and selection in relation to sex*. Vol. 1. London: Murray, 1871.

DATTA, L. G., MILSTEIN, S., & BITTERMAN, M. E. Habit reversal in the crab. *J. comp. physiol. Psychol.*, 1960, **53**, 275–278.

DETHIER, V. G., & BODENSTEIN, D. Hunger in the blowfly. *Z. Tierpsychol.*, 1958, **15**, 129–140.

DOLLARD, J., & MILLER, N. E. *Personality and psychotherapy*. New York: McGraw-Hill, 1950.

GALAMBOS, R. Potentials from the body wall of the earthworm. *J. gen. Psychol.*, 1939, **20**, 339–348.

HARALSON, J. V., & BITTERMAN, M. E. A lever-depression apparatus for the study of learning in fish. *Amer. J. Psychol.*, 1950, **63**, 250–256.

HARLOW, H. F. The formation of learning sets. *Psychol. Rev.*, 1949, **56**, 51–65.

HORNER, J. L., LONGO, N., & BITTERMAN, M. E. A classical conditioning technique for small aquatic animals. *Amer. J. Psychol.*, 1960, **73**, in press. (a)

HORNER, J. L., LONGO, N., & BITTERMAN, M. E. A shuttle-box for the fish and a control-circuit of general applicability. *Amer. J. Psychol.*, 1960, **73**, in press. (b)

HULL, C. L. The place of innate individual and species differences in a natural-science theory of behavior. *Psychol. Rev.*, 1945, **52**, 55–60.

KINNAMAN, A. J. Mental life of two *Macacus rhesus* monkeys in captivity. *Amer. J. Psychol.*, 1902, **13**, 173–218.

LONGO, N., & BITTERMAN, M. E. Improved apparatus for the study of learning in the fish. *Amer. J. Psychol.*, 1959, **72**, 616–620.

LONGO, N., & BITTERMAN, M. E. The effect of partial reinforcement with spaced practice on resistance to extinction in the fish. *J. comp. physiol. Psychol.*, 1960, **53**, 169–172.

MILLER, N. E. Comments on multiple-process conceptions of learning. *Psychol. Rev.*, 1951, **58**, 375–381.

PORTER, J. P. A preliminary study of the psychology of the English sparrow. *Amer. J. Psychol.*, 1904, **15**, 313–346.

SCHNEIRLA, T. C. Ant learning as a problem in comparative psychology. In P. L. Harriman (Ed.), *Twentieth century psychology*. New York: Philosophical Library, 1946. Pp. 276–305.

SKINNER, B. F. A case history in scientific method. *Amer. Psychologist*, 1956, **11**, 221–233.

SMALL, W. S. Experimental study of the mental processes of the rat. *Amer. J. Psychol.*, 1901, **12**, 313–346.

THORNDIKE, E. L. Animal intelligence: An experimental study of the associative processes in animals. *Psychol. Rev., Monogr. Suppl.*, 1898, **2**(4, Whole No. 8).

THORNDIKE, E. L. *Animal intelligence*. New York: Macmillan, 1911.

WATSON, J. B. *Behavior: An introduction to comparative psychology*. New York: Holt, 1914.

WODINSKY, J., & BITTERMAN, M. E. Partial reinforcement in the fish. *Amer. J. Psychol.*, 1959, **72**, 184–199.

WODINSKY, J., & BITTERMAN, M. E. Resistance to extinction in the fish after extensive training with partial reinforcement. *Amer. J. Psychol.*, 1960, **73**, 429–434.

YERKES, R. M. Methods of exhibiting reactive tendencies characteristic of ontogenetic and phylogenetic stages. *J. anim. Behav.*, 1917, **7**, 11–28.

Comment

Mowrer on "Sin"

In a recent issue of the *American Psychologist*, Hobart Mowrer (1960) argues that because "sin" is a stronger word than "wrongdoing" or "irresponsibility" it is better for the "neurotic" individual to admit his "sins" than accept his "wrongdoings." Says Mowrer:

> . . . the only way to resolve the paradox of self-hatred and self-punishment is to assume, not that it represents merely an "introjection" of the attitudes of others, but that the self-hatred is realistically justified and will persist until the individual, by radically altered attitude *and action*, honestly and realistically comes to feel that he now deserves something better. As long as one remains, in old-fashioned religious phraseology, hard-of-heart and unrepentant, just so long will one's conscience hold him in the vise-like grip of "neurotic" rigidity and suffering. But if, at length, an individual confesses his past stupidities and errors and makes what poor attempts he can at restitution, then the superego (like the parents of an earlier day—and society in general) forgives and relaxes its stern hold; and the individual once again is free, "well" (p. 304).

In upholding the concept of individual (if not original) "sin," Mowrer is contending that the "neurotic" individual must, if he is to get "well," accept the following syllogism: (*a*) sinning is unjustified; (*b*) I have sinned; (*c*) therefore, I must justify my existence by acknowledging my sins, changing my ways, and becoming a nonsinner.

At first blush, this seems like a perfectly valid syllogism. But, as Mowrer himself suggests, it rarely works in practice because

> There is some evidence that human beings do not change radically unless they first acknowledge their sins; but we also know how hard it is for one to make such an acknowledgment unless he has *already changed*. In other words, the full realization of deep worthlessness is a severe ego "insult"; and one must have some new source of strength, it seems, to endure it. This is a mystery (or is it only a mistaken observation?) which traditional theology has tried to resolve in various ways—without complete success. Can we psychologists do better (p. 304)?

I think we can. Let us first see what is wrong with Mowrer's syllogism and why, because of the manner in which it is stated, it virtually forces the individual fully to realize his deep worthlessness and his consequent inability to change his behavior. Mowrer's premise is that sinning is unjustified or that the sinner's "self-hatred is realistically justified." By this statement he appears to mean two important things, only the first of which can be objectively validated: (*a*) the sinner's act is mistaken or wrong (because it is, in some early or final analysis, self- or society-defeating); and (*b*) therefore, the sinner is personally blameworthy or integrally worthless for performing this mistaken or wrong act.

Although *a* may be a true observation, *b* is an arbitrary value judgment, or moralistic definition, that can never possibly be objectively validated and that, as Ellis (1958), Epictetus (1899), Hartman (1959), Lewis (1949), Mead (1936), and other writers have shown, is philosophically untenable. No matter how responsible, in a causative sense, an individual may be for his mistaken or wrong behavior, he becomes a villain or a worthless lout only if members of his social group *view* or *define* him as such and if, more importantly, he accepts their moralistic views.

The paradox, therefore, that Mowrer posits—that the "neurotic" sinner will not get better until he acknowledges and actively repents his sins and that he will not acknowledge his sins until he gets better—is a direct and "logical" result of our explicitly or implicitly including the concept of personal worthlessness in the definition of "sin." Naturally, if I believe that my acts are sinful—meaning that I am wrong (self- or socially-defeating) for perpetrating them *and* that I am blameworthy or worthless for being wrong—I will not *dare* acknowledge that I have sinned, or I will make invalid excuses for so doing, or I will feel so worthless after my acknowledgment that I am most unlikely to be energetic or effective enough to change my wrong or mistaken behavior.

How can the psychologist help his "neurotic" patients resolve this paradox? Very simply: by taking the objective and "weaker" (that is, unmoralistic) words, such as "wrongdoing" and "irresponsibility," that Mowrer abandons in place of "sin" and putting them into his original syllogism. The syllogism then becomes: (*a*) wrongdoing is self- or society-defeating; (*b*) I have made a mistake or committed a wrong act; (*c*) therefore, I had better stop being self-defeating by acknowledging my wrongdoing, take considerable time and effort to work at not repeating it, and eventually become a less frequent wrongdoer.

With this more objective and realistic restatement of Mowrer's syllogism, the problem of the "neurotic" individual's changing his ineffective and self-defeating behavior is hardly automatically solved; but (by having the definitional concepts of deep worthlessness and severe ego "insult" removed from his philosophic premises) he becomes much more likely resolutely to tackle what Mowrer accurately describes as his moral-psychological difficulties.

REFERENCES

Ellis, A. Rational psychotherapy. *J. gen. Psychol.*, 1958, **59**, 35–49.

EPICTETUS. *The works of Epictetus.* Boston: Little, Brown, 1899.
HARTMAN, R. S. *The measurement of value.* Crotonville, N. Y.: General Electric Company, 1959.
LEWIS, C. L. The nature of ethical disagreement. In H. Feigl & W. Sellars (Eds.), *Readings in philosophical analysis.* New York: Appleton-Century-Crofts, 1949.
MEAD, G. H. *Mind, self, and society.* Chicago: Univer. Chicago Press, 1936.
MOWRER, O. H. "Sin," the lesser of two evils. *Amer. Psychologist,* 1960, **15**, 301–304.

ALBERT ELLIS
New York City

As I read Mowrer's article on "sin" in the May issue, I found myself downright puzzled. If I understand him correctly, he is arguing that we should bring the concept of sin into psychotherapy for three reasons.

That, come to think of it, we are all sinners and the sooner we admit this elementary truth, the better off we will be.

The idea of the feeling of guilt being a neurotic phenomenon is fallacious (the analysts may believe so but they are on the way out anyway). It is not a displacement of infantile conflicts into the present, but a realistic appraisal of our sinful condition.

Bringing the idea of "sin" into the picture has a tactical advantage. Since the psychiatrists have mislaid the term somewhere, bringing it back puts us one up on them. This apparently is putting "sin" to good use. Since we cannot get rid of it, let us use it.

For my own part, I would like to differ fairly strongly with the first and second of these two points. The idea that man is a sinful animal has a long and illustrious history. Judeo-Christian theology is saturated with it and puts the blame on the shoulders of Adam and Eve—the prototypes of all parents.

Clinical practice confirms this. It is no novelty to say that the average patient suffers from a sense of guilt. With some it is a focal point in their symptoms. With others it is somewhere in the background. Freud would probably say that the existence of some guilt develops almost automatically from the structure of the home. All children wish to do things they are not allowed to do—sexual or otherwise. So they all feel somewhat guilty.

But granting the fact that guilt, for whatever reason, is part and parcel of the human psyche, what is the story on excessive guilt? We are all familiar with individuals who, leading outwardly blameless lives, torture themselves constantly with accusations of sin. Indeed, they cannot leave themselves alone, but constantly flagellate themselves for sins which the rest of us (lacking much character, perhaps) tolerate more or less and go about our business.

It is true that treating such individuals as real sinners makes them feel more content. Nor is it any secret that these people go out of their way to frequently provoke others—policemen, friends, wives, psychologists—to give them a tough time. However, it seems to me that acting out the punitive role with these people, telling them they are real sinners—while it may make them temporarily happier—brings them no closer to either understanding or changing themselves. It merely provides them with the rationalization they crave to avoid facing their problem. Yet, it seems to me, Mowrer is advocating just that.

After all, most clinicians are not concerned with the philosophical ramifications of "sin" and guilt, or with guilt as a theological problem. Whether guilt is a necessary part of the human condition is a very interesting point—perhaps related to original sin as the theologians would have it, or to incest fantasies as Freud would say—but in the clinic we are dealing with excessive guilt. This guilt is poisonous, corrosive guilt which destroys the patient's soul, leads him to suicide, murder, self-destruction, and the placing of the world's sins on his more or less innocent shoulders. No better term exists for this phenomenon than sickness—sickness of the mind, perhaps, but sickness none the less.

GOODHUE LIVINGSTON
Seattle, Washington

O. Hobart Mowrer's article on "Sin" . . . is a remarkable contribution in many respects. One could comment on many aspects of this impassioned plea for irrationality, but it seems prudent not to. It was common to misinterpret Freud's discoveries in the early days of psychoanalysis; it is regrettable that it occurs in 1960 and from the pen of a distinguished psychologist. Perhaps all this follows from Mowrer's pessimistic premise that "psychoanalysis, on which modern 'dynamic' psychiatry is largely based, is in a state of virtual collapse and imminent demise." This view undoubtedly reflects much wishful thinking, but it would be instructive to know more about Mowrer's sources of information.

It is really quite beside the point whether Freud's discoveries had the incidental effect of forcing the medical profession which had ignored his work to take note of it. The rules of scientific evidence are also given a somewhat novel twist in citing Katie Lee's 12-inch LP recording "Songs of Couch and Consultation." What is important, among other things, is Mowrer's persistent and egregious misinterpretation of Freud's conception of neurosis.

It should hardly be necessary to point out that one of the central ingredients of this conception is that the neurotic patient suffers from *unconscious* conflicts. Unconscious means just that: important strivings, the defenses used against them, and the constellations which gave rise to them in the first place are not acces-

sible to conscious awareness. Similarly, the patient's archaic superego, contrary to Mowrer's assumption, is not made up of the standards laid down or enforced by society. Parenthetically, this is beautifully brought out in Mowrer's own quotation from Freud's *Introductory Lectures*. Indeed, the archaic superego has very little to do with reasonable adult moral standards and the same applies to the infantile impulses ("transgressions") which are inveighed against. If this be true—and the clinical evidence is abundant—what sense does it make to speak of realistic attitudes and action? No neurotic patient "decides" to suffer or to hate himself (and others), any more than one decides to contract appendicitis or the common cold. By the same token, how can he make restitution or confess "sins," the nature of which he does not know? This is precisely one of Freud's great contributions which Mowrer is so eager to disparage: to have demonstrated and explored with infinite perspicacity and genius the dynamics of unconscious conflicts. Equally important, he provided an instrumentality (the psychoanalytic situation) to help the patient come to terms with his inner conflicts.

Freud does not say the patient is "right" and society is "wrong." Neither, however, does he say (as Mowrer repeatedly and fervently does) that society is "right" and the patient is "wrong." It is not a matter of right or wrong, nor is the solution of a neurotic conflict ever facilitated by blaming anyone. What seems to help is to enable the patient to face his conflicts. This is what therapy is about. Most psychotherapists, I venture to say, would agree that it would be of little avail to treat the patient who feels miserable and comes for help as a sinner, as Mowrer seems to advocate. A fair number would also agree that "unconditional positive regard" is not the total answer, either. But there is such a thing as understanding, respect for a suffering human being, and a willingness to help in constructive ways. Christian theology had a word for it: *caritas*.

Whatever the therapeutic effectiveness of psychoanalysis or other forms of psychotherapy may turn out to be in the long run, Freud had an abiding respect for scientific evidence and he had the moral courage to look at phenomena unflinchingly. The hope for psychology as a science, as I see it, does not lie in an alliance with religion or theology; it lies in a willingness to look at facts without moralizing about them and the patient pursuit of knowledge, wherever it may lead. In this direction Freud pointed the way.

HANS H. STRUPP
University of North Carolina

In his recent article, Mowrer (1960) suggested that it might be more advantageous to consider a neurotic as one who is "sinful" rather than as one who is "sick." In the present paper objection is being raised to Mowrer's formulation not solely on the basis of the obvious begging of the question with regard to the concept of sin, but, also, in terms of the fallacies of logic implicit therein.

For Mowrer, the idea that a neurotic is one who is *sick* is attributable to Freud and psychoanalysis, therefore, the way to abrogate the concept is to repudiate the theory. While Freud did consider the unconscious to be the major cornerstone of psychoanalytic theory, it was not the only one.[1] Mowrer, however, makes the assumption that "psychoanalysis" is synonymous with the "unconscious," and, hence, takes his stand unnecessarily against the theory *in toto*. In actuality, he could have confined his efforts to only one aspect of the theory.

Even so, Mowrer might have reviewed experimental data demonstrating that psychoanalytic constructs have little or no validity; he might have reflected on the difficulty in translating Freud's metapsychology into psychological analogs, or he might have pointed to modification in theory and technique. Rather, he summarily dismisses psychoanalysis by a sweeping generalization—". . . the fact is that psychoanalysis . . . is in a state of virtual collapse and imminent demise" (p. 302)—neither scientific in form nor informative in content, and a statement of dubious validity. Obviously, there have been modification and extension of the original theory and technique, consistent with Freud's expectations.[2] However, much of the altered point of view relates to such constructs as the oedipus complex [3] and what has been the apparent emphasis on the sexual in the theory of psychosexual development. The concept of the unconscious, though questioned (e.g., Mowrer, 1959, 1960), remains *experimentally* unchallenged. Psychoanalytic theory in general and, in specific, the construct of the unconscious is, e.g., still basic to much of the work done in many forms of psychotherapy, to the work done in research laboratories such as those of Murphy at the Menninger Foun-

[1] In 1922, Freud (1950a, Vol. V) expressed what he considered to be the cornerstones of psychoanalytic theory, viz., "The assumption that there are unconscious mental processes, the recognition of the theory of resistance and repression, the appreciation of the importance of sexuality and of the Oedipus complex . . ." (p. 122).

[2] "You know that we have never been proud of the fullness of our knowledge and our capacity; as at the beginning, we are ready . . . to admit the incompleteness of our understanding. to learn new things, and to alter our methods in any way that yields better results" (Freud, 1950a, Vol. II, p. 392).

[3] Anthropological research seems to caste doubt as to the universality of the oedipus complex, Mullahy's book (1948) indicates that there are differing opinions about the complex amongst theoreticians, and amongst some writings one sees an increasing emphasis on the *pre*oedipal situation in the development of neuroses, e.g., Brunswick (1940), Fenichel (1931), and Frank (1958).

dation and Klein at New York University, and is still the underlying principle to projective testing and psychosomatic medicine, to name but a few. All this, plus the ever-increasing demand for institute training by both medical and nonmedical therapists hardly bespeaks of the "virtual collapse and imminent demise" of psychoanalysis.

Another, though common, fallacy in Mowrer's formulation is inherent in the statement:

> So long as we subscribe to the view that neurosis is a bona fide "illness," without moral implications or dimensions, our position will, of necessity, continue to be an awkward one.

Were this true, Mowrer would be correct. However, here Mowrer may be criticized for fighting the proverbial straw man. Psychoanalytic theorists accept the interaction between culture and personality. Mowrer himself points out that Freud assumed that neurosis was due in part to the slings and arrows of an outrageous superego, and Freud's concept of the superego reflects the individual's attitudes towards and (in neurosis, inaccurate) perception of external prohibition. Freud also described the neuroses as the negative of the perversions, so that some moral framework is even implied here. But more important is Freud's (1950b) statement:

> Psychoanalysis has been reproached time after time with ignoring the higher, moral, spiritual side of human nature. The reproach is . . . unjust, both historically and methodologically. For . . . we have from the very beginning attributed the function of instigating repression to the moral and aesthetic tendencies in the ego . . . (p. 46).

Based on this, Fingarette (1955) wrote:

> . . . I am trying . . . to illustrate how psychoanalytic theory and therapy require the use of "guilt" and "responsibility" as essential psychoanalytic conceptions *while being used in a moral sense* [sic].

Finally, let us examine this concept which Mowrer proposes to substitute for neurotic illness. "Sin" has obvious similarities to Freud's concept of the id, viz., both are considered part of the constitutional makeup of the individual, hence both are, to a great extent, beyond the control of the individual. One may choose to give in to these tendencies, but both are motivational factors that run contrary to the existing social order and, as Mowrer points out, the consequence of indulgence in either may be hell (on earth, or elsewhere). Yet, Mowrer could be right; the neurotic may indeed have a sinful nature (!) especially if we regard infantile modes of expressing feelings of hatred and love as "bad." For if any of the research by Cattell, Eysenck, Greenacre, and Shirley, to name but a few, have any validity, the "original sin" of the neurotic would seem to be to have been born with a nervous and/or metabolic system that predisposes the individual to undue anxiety under stress.

Essentially, what Mowrer seems to be demanding is that man must come to accept and take responsibility for *all* of his ideas, feelings, and behavior. If this is true, then Mowrer is again waxing quixotic. Is this not what Freud intended in making the unconscious conscious, and is this, also, not the same point made by such as Ausubel (1955) and Shoben (1957)? Mowrer seems caught in the trap of dichotomous thinking, viz., either man's asocial behavior is motivated by unconscious factors, then he may be called "sick," or he is quite aware of what he is doing and just trying to "get away" with something, then he is "sinful." In thinking thus, Mowrer may be accused of having failed to make the distinction between phenotypical and genotypical behavior, and on the basis of fallacious logic, he expends his energy denying the undeniable, jousting at the ghosts of ideas no longer "alive." Surely few maintain that *all* of man's behavior is attributable to the unconscious, but neither can it be said that *none* of it is. In his haste Mowrer may be said to have thrown the baby out with the bath water. In fact, and in summary, fallacious logic has helped him overlook an interesting compromise offered by Erikson (1950): "I should like to suggest . . . an area between normalcy and deviation, the area of 'what you can get away with.'"

REFERENCES

AUSUBEL, D. P. Relationship between shame and guilt in the socializing process. *Psychol. Rev.*, 1955, **62**, 378–390.

BRUNSWICK, R. M. The preoedipal phase of the libido development. *Psychoanal. Quart.*, 1940, **9**, 293–319.

ERIKSON, E. H. Growth and crises of the healthy personality. In M. J. E. Senn (Ed.), *Symposium on the healthy personality*. New York: Josiah Macy, Jr. Foundation, 1950.

FENICHEL, O. The pregenital antecedents of the oedipal complex. *Int. J. Psychoanal.*, 1931, **9**, 141–166.

FINGARETTE, H. Psychoanalytic perspectives on moral guilt and responsibility: A re-evaluation. *Phil. phenomenol. Res.*, 1955, **16**, 18–36.

FRANK, G. H. On neurosis: Theory and treatment. *Psychol. Rep.*, 1958, **4**, 723–728.

FREUD, S. *Collected papers.* London: Hogarth, 1950. (a)

FREUD, S. *The ego and the id.* London: Hogarth, 1950. (b)

MOWRER, O. H. Changing conceptions of the unconscious. *J. nerv. ment. Dis.*, 1959, **129**, 222–234.

MOWRER, O. H. "Sin," the lesser of two evils. *Amer. Psychologist*, 1960, **15**, 301–304.

MULLAHY, P. *Oedipus, myth and complex.* New York: Hermitage, 1948.

SHOBEN, E. J., JR. Toward a concept of the normal personality. *Amer. Psychologist*, 1957, **12**, 183–189.

GEORGE H. FRANK
University of Miami

Psychology in Action

PSYCHOLOGY IN TURKEY:
SPECULATION CONCERNING PSYCHOLOGY'S GROWTH AND AREA CULTURES

FRED McKINNEY

University of Missouri

OVER the past few years several articles have appeared on the status of Psychology in various parts of Europe (Lehner, 1955; Razran, 1958; Summerfield, 1958; Zajonc, 1957). Reading these articles before I left the United States to serve as Fulbright lecturer in Turkey for the academic year 1958–59 led me to consider writing on the position of Psychology in the Turkish culture.

After the first few weeks in Turkey, I concluded that compared with the status of Psychology in some other areas of the world, research and professional activity in Turkey is still in its early stages and therefore it would be impractical to devote an article to it. However, as I became better acquainted with the problems and activities associated with the westernization of Turkey and neighboring countries, I saw that Turkey's development in Psychology might well be representative of a number of nations in the Afro-Asian bloc and possibly some in other areas. An article by Prothro and Melikian (1955) discussing Psychology in the Arab Near East, and a perusal of the *International Directory of Psychologists* further confirmed my feeling that an analysis of the conditions associated with the status of Psychology in Turkey would have value. I thereupon began to gather all available information about the history of Psychology and related disciplines in post-Ataturk Turkey (1923 *et seq.*) as well as to try to discover the degree to which the applied technologies fed by Psychology are needed in this rapidly expanding Middle Eastern democracy.

Academic Psychology in Istanbul

A brief note on the history of Psychology in Turkey (Istanbul and Ankara) appeared in a 1956 publication of the University of Istanbul edited by Walter Miles. It pointed out that Psychology in Turkey began in 1915 with G. Anschutz's arrival in Istanbul with several pieces of apparatus and a plan to establish Experimental Psychology at the university. This beginning was aborted with Anschutz's departure from Turkey after World War I, and little remained of his efforts. The real pioneer in Istanbul was a Turk, Shekip Tunch, who studied at the Institute of Jean Jacques Rousseau and returned to Istanbul in 1919 to fill the chair of General Psychology. During his career he translated several classics in the general field, including two of William James' books and one of Freud's.

In 1933 another attempt was made to establish a chair in Experimental Psychology but this failed because of the death of the appointee (a refugee from the Nazis) before he could reach Turkey. William Peters of the University of Jena, however, did establish in 1937 the Institute of Pedagogy which included the chair of Experimental Psychology and a functioning laboratory. Mumtaz Turhan, who had studied in Berlin and received a doctorate from the University of Frankfurt, had returned to his native Turkey in 1936 and joined in the advancement of psychological experimentation. His training included work at Cambridge, England, with Bartlett. He became Professor of Experimental Psychology in 1951.

The laboratory at the University of Istanbul published two volumes of studies from 1940 to 1952 and in 1956 Walter Miles, formerly of Yale University, edited the first volume of a projected annual publication entitled *Istanbul Studies in Experimental Psychology.* Frederick Charles Bartlett, of Cambridge University, was invited in the same year to deliver several lectures entitled "Some Recent Developments in Psychology in Great Britain," which were published in English and Turkish the next year in a university monograph. The second volume of *Istanbul Studies in Experimental Psychology* appeared in 1958.

Fulbright grants have also provided lecturers at the University of Istanbul: in 1952, N. H. Pronko, of the University of Wichita, served as Professor of Experimental Psychology; in 1957–58 E. P. Hollander, then of Carnegie Tech, was Visiting Professor of Psychology with particular emphasis on empirical Social Psychology. In 1958–59, Homer B. Reed, formerly of Fort Hays Kansas State College, was Visiting Professor of Experimental Psychology.

The laboratory has produced several doctoral investigations and many minor studies in the recent past.

At present studies are underway in the areas of visual perception and of interpretation of emotional expressions. Four assistants work in this division and a chair of Applied Psychology has been approved by the university but no appointment has as yet been made. Several graduates from the laboratory are at present continuing their graduate work in the United States and England and expect to return to Turkey to help fill the demand for psychological services that is developing in clinical, educational, industrial, and military areas. In addition to the experimental activities there is also the chair in General Psychology occupied since Tunch's retirement by S. E. Siyavushgil who has translated into Turkish several of Piaget's books. Additional staff consists of a dozent (assistant professor) Refia Ugurel Shemin who adapted the Army Alpha test to Turkish culture and assistants.

ACADEMIC PSYCHOLOGY IN ANKARA

Academic Psychology in Ankara (Dil ve Tarih Cografya Fakultesi) had promising early leadership under Muzafer Sherif, well known today in the United States. The APA *Directory* shows Sherif received an MA from the University of Istanbul in 1929, an MA from Harvard University in 1932, and a PhD from Columbia University in 1935. He was appointed Assistant Professor at Gazi Institute (the outstanding teacher-training institute in Turkey) and served there from 1937 until he became Assistant Professor of Psychology in the new Arts faculty at the University of Ankara. Sherif translated several important American publications among which are the Terman and Merrill *Manual of the Binet Test* and Woodworth's *Contemporary Schools of Psychology*. He left Ankara in 1945, has been actively publishing in America since that time, and is at present a member of the faculty at the University of Oklahoma. Since 1945 the position at Ankara has been occupied by visiting Americans, beginning with C. C. Pratt, now at Princeton, who established an institute of Philosophy and who remained at Dil ve Tarih Cografya Fakultesi until 1947; T. W. Reese, now at Mount Holyoke, was a Fulbright grantee in 1951 and 1952; John Volkman, also now at Mount Holyoke, followed him in 1952–1953. Volkman was succeeded by Spaulding Rogers, a Smith-Mundt grantee who remained in Turkey until 1957 and is now at Southern Illinois University.[1] Mark K. Allen, of Brigham Young University, filled the chair for the academic year 1957–58 and the author the following year; the chair is presently unoccupied, even though the Fulbright Commission made available to the faculty a reputable educational psychologist, a Fellow in APA. I have been unable to learn the technicality that blocked the appointment in the final stages. It seems to add evidence for the hypothesis that the Turkish culture presents difficulties to the growth of Psychology. The work of several of these men has been somewhat integrated in that Husnu Ciritli, who teaches Tests and Measurements at Gazi Institute, acted as interpreter for their lectures. Ciritli pursued graduate work at several American universities and has adapted for the Turkish reader material in book form on Tests and Measurements and on Educational Psychology.

The first 10 chapters of Norman Munn's *Psychology* (second edition, 1946) have been available since 1954 in Turkish translation rendered by a Philosophy teacher. In 1958 the same translator, Nadid Tendar, completed his work. All illustrations are reproduced in the Turkish edition, giving the student who has only Turkish at his command a good introduction to Western Experimental Psychology. It is my impression that the book is not nearly so popular in translation as a Westerner might expect.

Despite the fact that the University of Ankara has throughout a decade had the benefit of these mature and productive Americans, each of whom left some legacy in the form of books, journals, apparatus, seldom-used equipment, and valued memories in the recollections of some Turks, a strong unit of Psychology and a tradition based on a group of graduate students and ongoing scholarly activities have not developed here as in other divisions of Dil ve Tarih Cografya Fakultesi. Many colleagues in the faculty who are desirous of a strong psychological tradition attribute its failure to develop to the short tenure of the visiting professor. Whether this is the major reason or whether some other factors to be discussed later are more strongly operative is an open question.

One of Psychology's greatest contributions to Turkey is somewhat indirect, resulting from the activities of visiting and foreign-trained resident psychologists at the training school for teachers and school supervisors at Ankara called Gazi Institute. Although the universities teach only 35 to 70 students Psychology during a year, normal schools, especially Gazi, reach hundreds of future teachers through courses in General, Child, Tests and Measurements, Mental Hygiene, Educational, and Clinical Psychology. The Ministry of Education presently utilizes the teachers of Psychology at Gazi and elsewhere during the summer months to arrange seminars for Turkish teachers in Adolescent Psychology, Guidance programs, and Educational Psychology. Then too, all lycée students in Turkey have a short general course in Psychology, at present more philosophically than experimentally oriented, and the stu-

[1] I am indebted to Spaulding Rogers for supplying some important details concerning records not otherwise available. Hasan Tan and George W. Angell, both of Ankara, and E. P. Hollander, of Washington University, also read the first draft of this paper and added points of fact.

dents in the American "colleges" (high schools) study a course very similar to our introductory course, using a popular American text. One of the early psychologists at Gazi was the late Egon Brunswik who came from Austria and later went to the United States where he is well known through his publications.

Delton C. Beier, of the University of Indiana, spent a year at Gazi and developed an individual intelligence test which is presently used. Hasan Tan, a Turkish psychologist who received his doctorate at the University of Maryland, is now teaching Psychology at Gazi. He has recently translated *Psychology and the New Education* by S. L. Pressey and Frances Robinson.

SPECULATION ON PSYCHOLOGY'S SLOW GROWTH

The question I have continued to ask myself and others throughout my residence in Turkey is why Psychology has *not* taken root as a science and technology despite its relatively early and significant beginnings both in Istanbul and Ankara, and despite the continual transfusion from the West in the form of especially well-qualified psychologists.

If we should compare Psychology in Turkey with Psychology in the northern countries as reported by Albee (1955), we should see that its vitality and systematic flavor have yet to develop. Among Turkish psychologists (unlike those in the northern countries), American journals are not widely read or digested. Student selection cannot as yet be as rigorous as it should be to produce a group of advanced students who can stimulate each other and will do individual reading and research. As in the northern countries, communication between psychologists or between psychologists and related scholars is negligible as compared with that in the West. Although there is a psychological association in Turkey, I am told it is an organization in name only.

Psychology in both universities is *situated* in what we call a College of Arts and Letters and closely related to Philosophy rather than to either biological or social sciences and more seriously, there is practically no interdivisional exchange in terms of course election. Professors in both universities regret the fact that Psychology attracts too few highly intelligent, motivated, and scholarly students. The better students gravitate in greater number to more established fields in which they can find prestigious and challenging employment. There is little doubt that if Psychology in the universities were more closely related to the technologies, social sciences, or education, and if the leaders in these fields saw how psychological insights and methodology could broaden and enhance their contributions to the country's development, it would have grown more before now. Where it has grown in Turkey and in other parts of the Middle East, academic Psychology has achieved a close relationship with services in the schools, welfare institutions, and government service. Page (1959) for example relates Psychology's importance in Egypt with a national policy directed toward increased human productivity; and Wickert (1960) indicates that whereas there are no psychology departments in about 33 African colleges and universities, of the 183 total psychologists, 62 are industrial psychologists.

Is the slow growth in Psychology due to some *resistance to it in the Islamic culture,* similar to that found earlier in Catholicism to empirical, nonscholastic Psychology? Although a deep-lying cultural resistance seems to exist, there is no indication that it is specifically related to contemporary religious attitudes. The Koran emphasizes learning and enlightenment and the educated Muslim seems more like the Protestant Christian or the reformed Jew than like the more orthodox Christian.

The notable difference in enthusiasm of the East and West for poetry and proverbs noted by Bowra (1958) and obvious to visiting professors offers another hypothesis. Is the need for understanding of human experience and behavior satisfied traditionally and effectively through this strong *affinity for the poetic* rather than through a more objective approach? This is doubtless one factor.

Other factors in the Muslim culture and Ottoman history that are still a part of the Turkish soil despite its avowed commitment to Westernization may discourage Psychology's taking root. *Authority* and the power vested in those in high office is still much stronger in this nation than in the West, despite its avowed democratic ideals. Further, individual and grass roots initiative has yet to develop strength. A *fatalistic surrender* to existing external forces is a cultural trend the Turks of Ataturk's generation and those since have had to fight. These attitudes are somewhat antithetical to individual responsibility and efficiency.

Many of the factors mentioned above contribute to an explanation for the comparatively slow growth of Psychology in Turkey, but most important probably is the fact that *Psychology is an expression of a certain aspect of the industrial-commercial Western culture.*[2] Turkey, with a strongly conscious Western motivation and accomplishment among its leadership has a short

[2] Some evidence of the relationship of psychology's development to the industrial-commercial complex in Western culture is noted as one compares by country the number of psychologists listed in the *International Directory of Psychologists.* Aside from the United States (not listed and far ahead in number of psychologists) the rank order of listings is Germany, Japan, the United Kingdom, and Canada. There is a gap in frequency, then the list continues with France, USSR, Australia, and the Netherlands. These nations top the list of the 70 included.

though vigorous history of Westernization which favored the *technological* aspects of the West. On the other hand, equally strong Eastern, pre-Ataturk attitudes mentioned above operate even among students and affect the acceptance of modern Psychology as an approach to understanding of human behavior. The results are that engineering ventures have leaped ahead of the technologies and sciences affecting the behavior of man (including medicine, it seems.) Evidence for a greater acceptance of Psychology as a technology in a Westernization program than as a theoretical discipline is seen in a more extensive development in a shorter time at the English speaking Middle East Technical University than at the University of Ankara, both located in Ankara.

TURKISH PSYCHOLOGY OF THE FUTURE

Climate of Opinion and Values of Young Turks

In terms of Allport's (1957) analysis of European and American psychological theory, Turkey today seems to reflect much more of the European climate of opinion though in technical areas it looks to America. There seems to be little enthusiasm among the present students for positivism or brain models, rather explanations in terms of the whole person seem to win favor. Today's Turkish youth reflect Islamic-European influences guided by strong admiration for Ataturk's pragmatism. They are in touch with American youth through the importation of our mass media though they seem to resemble more the European student than the American in values and introverted orientation as described by Gillespie and Allport (1955). Their attitudes and behavior do not strongly reflect "mutability, reactivity, and meliorism" (Allport, 1957).

Contemporary Developments in Applied Psychology

Already in Istanbul and Ankara there seems to be a steadily increasing recognition of need and demand for well-qualified psychologists not only in schools but in medical, welfare, government, and military institutions. A laboratory of Psychotechnique has been set up at the Traffic Bureau in Istanbul, another at the Railway Department. A similar laboratory has been established at the Training College for Technical Schools in Ankara. The Ministry of Education has established a testing service for the school system of Turkey at Ankara. George W. Angell, Jr., who received his degree in Measurement at Boston University, has been consultant for this service since 1956, working first under a Smith-Mundt grant, then on ICA appointment. He was preceded by George A. Prescott with a doctorate from Boston University, and later by Claude F. Bridges. Feriha Baymur, who now is Director of the bureau, just returned from the University of Illinois where she went on leave to obtain a doctorate in Counseling. Recently Kenneth Nelson, a Minnesota doctorate, has been appointed to the service as research consultant. This service, since its establishment six years ago by William Kvaraceus, a Harvard doctorate, has done much to acquaint teachers with the value and uses of psychological tests in education. It is at present developing achievement tests and assisting Turkish lycées and institutions of higher learning in the selection of students, standardizing tests at various educational levels.

Some interesting activities related to Psychology have been carried on by visiting researchers connected with the Faculty of Political Science of the University of Ankara. Fred Frey, a Rhodes scholar and Ford Foundation grantee who is working toward a doctorate at Princeton, has launched a nationwide study of the values of Turkish college youth. Betty Shuey, a social worker, has established a training nursery school at the College for Women in Istanbul. The Dean of Students at Robert College in Istanbul, A. Allen, is a PhD in Psychology from the University of California. The newly organized Middle East Technical University, which follows the American educational pattern, not only uses the part-time services of the Fulbright grantee but also has added American-trained Turkish psychologists Hasan Tan and Mitoit Ench, with an MA in Education from Columbia University and a doctorate from the University of Illinois, to the staff. Social work seems even less developed than Psychology. Psychiatry, without great emphasis on psychotherapy, is developing, especially in Istanbul, but has practically no contact with Psychology.

Prothro and Melikian's (1955) favorable prognosis for Psychology in the Arab world might well hold for Turkey and those other nations which are moving toward greater urbanization, industrialization, mass production and distribution, changes in social institutions, and increase in literacy and per capita income—all of which should stimulate an increased use of psychological understanding and technology. These countries will be looking toward the West and particularly the United States for its store of research findings, its professors who can spend a year or more with them, assistance for students who need advanced study abroad, and finally, for books and periodicals. The Westerners who respond to these needs must approach with an understanding of the cultural complex into which they are moving and realistic notions of the rate and kind of progress they can make. Otherwise, they will feel frustrated and fail in their efforts to help the country to which they go.

Turkey and its neighbors, in return, can provide the Western world contact with aspects of a culture which developed centuries ago and has nourished its people as affectionate, patient, understanding, and cultured

citizens of the world. Living in a milieu with its heart in the East and its mind toward the West can throw in relief the total Western value system (including that of science) for more careful scrutiny, evaluation, and for integrative development.

REFERENCES

ALBEE, G. W. Psychology in the northern countries. *Amer. Psychologist*, 1955, **10**, 82–83.

ALLPORT, G. W. European and American theories of personality. In H. P. David & H. V. Brackew (Eds), *Perspectives in personality theory*. New York: Basic Books, 1957. Chap. 1.

BOWRA, C. M. Poetry and tradition. *Diogenes*, 1958, **22**, 16–26.

GILLESPIE, J. M., & ALLPORT, G. W. *Youth's outlook on the future*. New York: Doubleday, 1955.

JACOBSON, E. H. (Ed.) *International directory of psychologists*. Netherlands: Royal Van Gorcum, 1958.

LEHNER, G. F. J. Psychological training facilities in Austria and West Germany. *Amer. Psychologist*, 1955, **10**, 79–82.

MILES, W. R. (Ed.) *Istanbul Universitesi tecrubi psikoloji chalishmaeari*. Vol. I. Istanbul: Baha Matbassi, 1956.

PAGE, H. E. Psychology in Egypt. *USN ONR tech. Rep.*, 1959, No. ONRL-24-59.

PROTHRO, E. T., & MELIKIAN, L. H. Psychology in the Arab Near East. *Psychol. Bull.*, 1955, **52**, 305–310.

RAZRAN, G. Psychology in communist countries other than the USSR. *Amer. Psychologist*, 1958, **13**, 177–178.

SUMMERFIELD, A. Clinical psychology in Britain. *Amer. Psychologist*, 1958, **13**, 171–176.

WICKERT, F. R. Industrial psychology in Africa. *Amer. Psychologist*, 1960, **15**, 163–170.

ZAJONC, R. B. Psychology in Poland. *Amer. Psychologist*, 1957, **12**, 730–733.

THE PSYCHOLOGIST AS EXPERT WITNESS:

A CASE REPORT AND ANALYSIS OF PERSONAL EXPERIENCES PREVIOUSLY REPORTED IN THE *AMERICAN PSYCHOLOGIST*

JACK ARBIT

Northwestern University Medical School

THE complementary articles by Schofield (1956) and McCary (1956), the former a general discussion of the psychologist as an expert witness in courts of law, his interactions with attorneys in this situation, and the potential pitfalls to the psychologist, the latter a technical report of legal findings and opinions in a number of civil and criminal, state and federal court cases in which psychological testimony was involved, have presented an excellent introduction to the psychologist interested in the problems of testifying as an expert witness. In addition, McCary (1960), in the form of a hypothetical trial transcript formed from transcripts of three actual court cases, reports nearly the entire range of problems the psychologist as a witness may encounter.

Legalistically oriented reviews of court cases in which psychologists have appeared and a discussion of various state and federal policies and decisions regarding testimony, privileged communication, etc. may be found in Louisell (1955, 1957, 1958). These reviews, in the main, are concerned with *clinical* psychological testimony as opposed to the testimony of social psychologists in, for example, desegregation cases. The problems of these two groups of psychologists testifying as expert witnesses are similar only in part: in his critique of the thesis that expert psychological testimony had a marked bearing upon the desegregation verdict of the Supreme Court, Cahn (1954, 1955) does not even raise the question of the psychologist as an expert while this is a most common experience of the clinician in court.

The present note will concern itself with the clinical psychologist as a court witness as revealed in the personal experiences reported in the *American Psychologist* with a single listing of references to these "case histories" as well as to the legal references previously scattered unsystematically in this journal. This analysis will include a discussion of the author's recent court experience.

The particular case to be reported here concerned a petition for bankruptcy in a United States District Court (Northern District of Illinois, Eastern Division: In Bankruptcy No. 58 B 8226; Vacating order entered April 22, 1959). The individual in question, in 1958, borrowed money from a loan company at which time, and subsequently, he failed to indicate that as financial secretary of a union local he was responsible for a monetary shortage and was in the process of making restitution to a bonding company for this loss. Ten months after making the loan bankruptcy proceedings were instituted by this individual. Because of the omission noted above, the loan company filed a petition for the denial of discharge of debt. A hear-

ing was held and a ruling made in favor of the loan company.

At this time the bankrupt petitioned for a rehearing of the loan company's objections on the basis that at the time of the loan the bankrupt lacked the mental capacity to prepare and execute a financial statement. In support of this argument to the Federal Court, a petition was filed and a decree entered in Probate Court declaring the bankrupt incompetent and a conservator was appointed to handle his financial affairs. On the basis of this additional evidence a new hearing of the bankruptcy petition was ordered.

At this juncture I was called into the case by the loan company with a request for a psychological evaluation of the bankrupt with particular reference to his sanity some 20 months earlier. I had no information about this individual other than the inferences I drew from a knowledge of the source and nature of the referral; in addition, I had at no time during the proceedings any of the usual case history material. The attorney for the bankrupt was present throughout the approximately six hours of testing, but remained unobtrusively in the background and appeared not to interfere with the test procedures. A report was submitted to the attorney for the loan company noting that at the time of testing the bankrupt showed signs of organic brain damage possibly due to a history of alcoholism and reacted in a manner frequently associated with schizophrenic disorders, for example, disturbed thought processes and a failure at reality testing. However, this appeared to be an individual who could make an adequate adjustment to the world on a simple level: he could perform simple work; engage in simple interpersonal relationships; and handle adequately his needs in terms of food, clothing, and shelter. I was then informed of the legal aspects of this case and requested to testify as to my findings in the upcoming court hearing.

The hearing was held and several lay witnesses testified as to their knowledge of the bankrupt's mental condition; in addition, the librarian of a hospital wherein the bankrupt had been hospitalized on several occasions with what appeared to be acute alcoholic episodes read excerpts of the physicians' notes into the record. Neurological examinations during these hospitalizations were "unremarkable." A psychiatrist then testified regarding his examination and finding that the bankrupt was mentally incompetent due to brain deterioration produced by an alcoholic condition. Cross-examination centered about the fact that if the bankrupt could handle his job, living requirements, medical and legal affairs, why is he incompetent when he borrows money from a loan company? Also, emphasis was placed upon the fact that the psychiatrist's evaluation was based almost solely upon what the bankrupt told him and the possibility that this might be somewhat biased considering the legal difficulties in which he was involved.

I was then called as a witness and immediately after my education and experience were noted the admissibility of my testimony as an expert was questioned by the bankrupt's attorney who emphasized my lack of a medical degree. The court ruled that as a psychologist I was competent to discuss the findings of psychological tests and thus could testify as an expert witness. An additional objection was then raised: by testifying without the bankrupt's consent I would be violating a confidential patient-doctor relationship. The court ruled that since I was not a physician I did not have a privileged relationship with the bankrupt and therefore could testify as to my examination. Cross-examination attempted to indicate that only a physician could testify as an expert concerning organic brain damage (the attorney used the phrase "organic brain *disease*") and that all schizophrenics were incompetent. Subsequently the psychiatrist was again called and testified that schizophrenia was like pregnancy—there were no degrees; and, although *he* did not find the bankrupt schizophrenic that if he were, as I had noted in my testimony, he would have a "split personality" and "these individuals are definitely mentally incompetent and are not responsible for their actions." Emphasizing the psychiatric testimony and the finding of mental incompetency in the Probate Court, the District Court ruled for the bankrupt allowing him discharge of his debts.

Although the case discussed here is concerned with bankruptcy proceedings (and I am not aware of a previous case of this nature in which a psychologist testified), there are many similarities with those instances already reported by psychologists. For example, Shoben (1950) was faced with the choice of either testifying or facing contempt of court charges because his argument that communications between himself and the defendent were privileged was rejected by the court. In the case reported here we also see this question arise, although in a somewhat different context, and a ruling similar to that in the Shoben report handed down. This question appears to be one which arises relatively infrequently but upon which there is some unanimity of opinion.

A more frequent occurrence is the objection raised to the psychologist testifying as an expert witness. McCary (1956) and Louisell (1955, 1957) discuss the legal precedents thoroughly. In the present case, as well as those reported by Frank (1956), Schofield (1956), Stopol (1957), Weitz (1957), and McCary (1960), the psychologist was allowed to testify as an expert. In one instance (May, 1956) the psychologist's testimony was excluded by the trial judge on the

grounds that the psychologist was not a medical man, but a Court of Appeals reversed this decision. Individual differences do occur in these cases: for example, in Schofield's report the psychologist was allowed to cite specific factual observations from test behavior and responses, but could not interpret these data in terms of a statement as to sanity or psychosis. The case reported in which psychological testimony was accepted most readily (Frank, 1956) concerned testimony as to the extent of the organic and personality disturbance produced in an accident by the defendent who had already been designated solely responsible.

Only in the case reported by Eisen (1953) was the psychologist barred from testifying as an expert witness, and since his testimony was then as a layman he could not report on the defendent's responses to psychological tests. Interestingly, the present case was conducted in Illinois as was Eisen's and considering that federal courts tend to adopt the rulings of the state in which they operate, the discrepancy in these two instances is quite sharply delineated.

It appears that in terms of measurement and evaluation based upon psychological tests, psychologists are accepted in the main as expert witnesses. This tends to be related to the acceptance of social psychological studies of the effect upon personality, as measured by various types of tests, of, for example, foreign languages taught in grade school and desegregation (Kendler, 1950). In regard to statements as to the implications for insanity or psychosis of these behavioral and test observations, there is some support for the view that psychologists are *not* allowed to testify. There was no objection in the present case however to the statement that the bankrupt's test responses were similar to those frequently obtained from schizophrenics.

McCary (1960) notes the possible use of the technique of derogating the psychologist by attacking his use of the title "doctor." Schofield indicates that this may be done either intentionally or in honest error through the attorney's own equating of the term doctor with physician. The author's experience is similar to that of Stopol in which there was no overt harassment but a subtle attack through the frequent interspersion of questions as to the psychologist's lack of a medical degree and inability to treat medical disorders some time after these facts had been well established.

McCary (1956) implies that certification or licensing of psychologists may solve the problems of the psychologist as an expert witness. This appears to be an oversimplification, for, as we have already noted, limited acceptance is already here. The difficulties encountered, probings, questionings, and the search for definitive and uncontradicted statements of fact once one is on the witness stand appear to be valid legal techniques for establishing facts and their validity. At present it appears of somewhat greater concern that our licensing or certification bills should not hinder the acceptability already obtained by the psychologist in the courts or the opportunity of the psychologist to evaluate individuals involved with the law.

Although others do not report this circumstance, the somewhat less than ideal conditions surrounding my examination (the bankrupt's attorney was present) and evaluation of test results (due to unavailability of background and developmental information) may be expected on occasion and the psychologist is cautioned to take whatever action is deemed necessary to avoid an exceedingly difficult situation.

In the present attempt to accumulate cases and reports a number of difficulties arose. For example, only the articles by Schofield and McCary and the cases reported by May and Frank were abstracted in the *Psychological Abstracts;* the other case history reports had to be obtained through search of the "Comment" and its predecessor, the "Letters to the Editor" section in the *American Psychologist.* Other references were found in the "Notes and News" as incidental items of general information. Possibly the APA Committee on Relationships with Other Professions enlisting the aid of psychologists and lawyers interested in this area might attempt to accumulate references to the widely spread literature including, in addition to the case history reports of clinical and social psychologists and reviews published in the legal literature, such diverse items as Wolfle's (1947) testimony as to the fraudulence of certain mail order personality courses.[1] The psychologist's reports are extremely valuable for they frequently communicate a great deal more than the sometimes barren and cold legal opinions whose value derives solely from the fact that they *are* legal decisions.

Lastly, psychologists reporting experiences should take care to give reference to courts, dates, case number, etc., so that specific incidents may be traced to obtain additional information. If generally available the cost of transcripts may be noted for those interested in complete reports—this latter, possibly, for research purposes.

REFERENCES

CAHN, E. Jurisprudence. In R. B. McKay (Ed.), *1954 annual survey of American law.* New York: Oceana, 1954. Pp. 809–828.

CAHN, E. Jurisprudence. In R. B. McKay (Ed.), *1955 annual survey of American law.* New York: Oceana, 1955. Pp. 655–667.

[1] To some extent this has already begun on the state level. A recent note in the *American Psychologist* (1960, **15**, 238) refers to a paper entitled "The Psychologist as a Witness in the California Court."

EISEN, N. H. Testifying as an expert witness. *Amer. Psychologist*, 1953, **8**, 595–596.

FRANK, I. H. Psychological testimony in a courtroom. *Amer. Psychologist*, 1956, **11**, 50–51.

KENDLER, T. S. Contributions of the psychologist to constitutional law. *Amer. Psychologist*, 1950, **5**, 505–510.

LOUISELL, D. W. The psychologist in today's legal world. *Minn. law Rev.*, 1955, **39**, 235–272.

LOUISELL, D. W. The psychologist in today's legal world. Part II. *Minn. law Rev.*, 1957, **41**, 731–750. (A third report in this series is to be published.)

LOUISELL, D. W. Increasing importance of psychology to law. *Va. law Wkly.*, 1958, **18**(6).

MCCARY, J. L. The psychologist as an expert witness in court. *Amer. Psychologist*, 1956, **11**, 8–13.

MCCARY, J. L. A psychologist testifies in court. *Amer. Psychologist*, 1960, **15**, 53–57.

MAY, R. A psychologist as a legal witness. *Amer. Psychologist*, 1956, **11**, 50.

SCHOFIELD, W. Psychology, law, and the expert witness. *Amer. Psychologist*, 1956, **11**, 1–7.

SHOBEN, E. J., JR. Psychologists and legality: A case report. *Amer. Psychologist*, 1950, **5**, 496–498.

STOPOL, M. S. A recent court experience. *Amer. Psychologist*, 1957, **12**, 42–43.

WEITZ, R. D. An expert witness. *Amer. Psychologist*, 1957, **12**, 42.

WOLFLE, D. Psychological frauds. *Amer. Psychologist*, 1947, **2**, 27–28.

Psychology in the States

Ethics and Ethos

In one of Stephen Potter's books on gamesmanship, he describes the ploy used by one golfer to render his colleague a hopeless duffer. It consists merely of asking, with appropriately feigned innocence, such a simple question as: "Do you always give your right elbow that little twist as your club meets the ball?" Said just as the other steps up to tee off, it is guaranteed to play hob with his game, if not with his life. So Potter says, and we care neither to test the technique nor feel its sting.

Maybe it does get tried on psychologists, though. Frankly, we sometimes cannot tell whether this or that magazine article is meant to twit us, acquit us, or honestly permit us to take a second look at how we play our own serious game. But that is no doubt *our* problem. We would only submit that there may be as much to be learned about where psychology is going by looking at what it does wrong as at what it does right. Peering into the looking glass of ethics may yield only mirror images, but if one worries less about which side is right or left, more about which is right or wrong, and most about the moral of the story, as it were, the reflection may be a valid one nonetheless.

Background. Such reflection is, if nothing else, timely. State associations are fast coming alive to problems of ethics on the local scene; the APA Committee on Scientific and Professional Ethics and Conduct (CSPEC) has within the year enlisted the good services of several state ethics committees; the Committee on Ethical Standards of Psychologists, which is expected to measure the adequacy of the present code partly in terms of its ability to encompass the problems of today, reports through the Board of Professional Affairs.

In all of this, APA, unlike the prophet, is not without honor in its own country. Thanks to the efforts of those who drafted and redrafted its code and those who struggle to enforce it, the association is coming to be seen as highly ethics conscious, if not wholly knowledgeable. Within recent months, members of CSPEC have been asked to participate in programs of the Society for Applied Anthropology and the National Education Association as each in turn strives to bring its own code into phase with the contemporary problems it is forced to confront.

Sometimes the shoe is on the other foot. Where the APA committees are writing guidelines to govern ever-emerging new problems, they are not averse to seeking the counsel of others. Thus, CSPEC has recently been in touch with its opposite numbers in the American Medical Association, the American Psychiatric Association, and the American Bar Association, offering to compare notes on how certain problems get handled.

Foreground. If the problems of information theory, motivation, and learning are complicated, those of ethics are no less so. And if anyone is confronted repeatedly with that harsh fact, it is, perforce, the committees which must satisfy their consciences that they are proceeding in all good faith and with all due reasonableness. Theirs is less the pleasant task of selecting interesting hypotheses for testing, more the worrisome one of dealing justly and intelligently with problems forced upon them.

All the while, both committees hear from APA members willing to think about problems of ethics and cognizant of the responsibilities here involved. The committee should take a more aggressive role, say some; it should proceed with great caution, suggest others. In this case the national association should intervene, exhorts APA member *X*; if ever there was a case that is best handled at the state level, this is it, counters member *Y*. The code is clear on a certain issue, insists one; his equally well-intentioned colleague urges the committee to consider seriously the rewording of the relevant principle to make it less ambiguous.

The committees listen well and earnestly. They would be among the last to claim either infallibility or the wisdom of lesser Solomons. Mayhap they take some comfort from the fact that the meeting of the Society for Applied Anthropology included even a philosopher among its panelists. Be that as it may, CSPEC sees its role as primarily educative. If in the process of carrying on its work the committee itself becomes educated on many counts, this is how it would have it.

Retrospect. When, in a memorable address (see the November 1958 issue of this journal) as EPA

President, Stuart Cook attempted to resolve the dilemma of whether psychology is to be a science and/or profession, we thought he made a masterful attempt. As we look at the report of issues dealt with by CSPEC during the past year, we wonder again whether problems of ethics may not, from quite another perspective, present a reasonable mirror image of psychology's development. At least we are struck by the fact that the matters at issue are hardly all of a "professional" character, that many of them are no less of the kind the scientist is very much heir to.

In short, the CSPEC report lists problems dealing with many an aspect of tests—their construction, interpretation, and use, their distribution, and the purposes they are intended to accomplish. There are questions involving the claims which may rightfully be made for certain psychological techniques in relation to presently available knowledge. There are matters of book reviews, the manner in which survey results are presented, the responsibilities owed research subjects, the representation of psychological techniques in public media, the right to discuss controversial issues in the classroom, and the amount of additional training needed to cross from one area of psychology into another. And there is many another issue which one would be hard put to assign to the professional as against the scientific category.

The whole of it seems less a record of transgressions than an inventory of the kinds of situations, roles, and impasses which history has bequeathed to the psychologist of 1960 to face as he will. False modesty aside, there is no presumption on the part of the committees directly concerned that they have the answers or hold exclusive rights to the wisdom here required. Counsel is welcome. Even criticism.

And especially help. Hence, the Committee on Scientific and Professional Ethics and Conduct takes this occasion to thank in particular the members and committees of several state associations which have, during the past year, helped share the responsibility for some decisions reached and some actions taken. Thanks go especially to California, New Jersey, New York, and Wisconsin.

* * *

***Et tu*, Demography.** We do not rightly know whether this lead requires the ablative, or, if it does, whether one has a right to mix Latin and Greek anyway. We do know that demographic studies, in any case, are becoming the order of the day. Perhaps it is in the tradition of Edwin G. Boring and his delightful incursions into the realms of biotropy, sociotropy, and psychotropy generally. Or else the more recent analysis of *America's Psychologists* by Kenneth E. Clark has sparked interest in a study of *Homo Psychologiens* (and here we would welcome any assistance from the grammarians among our readers).

Whatever the provocation, demographers are rising in our midst. Several earlier surveys have been reported in this column. Here we want to mention such more recent efforts as those of E. Lowell Kelly, who, as President of Division 12, took a long and considered look at his divisional colleagues; the survey of Illinois psychologists conducted by Dik Twedt; and the study of Pennsylvania's private practitioners presented by Murray Levine in a symposium at the Chicago convention.

The APA Central Office is not unaware of the contributions it in turn can make. Data from the National Science Foundation Register of Scientific and Technical Personnel are now ripe for analysis, a preliminary survey of the experiences of psychologists in legal situations has been reported earlier in this column, the data contained in the files of the ethics committee will receive close scrutiny during the coming year.

In this business of demographic studies, the *deus ex machina* may well prove to be some new equipment to replace the clanking gears which presently labor solely to grind out the innumerable mailings to APA members and journal subscribers. Given more versatile robots, vintage 1960, the Central Office will be in a position to feed into them urgent questions in the hope of getting cogent answers.

* * *

Words Will Not Be Minced. A truck used to run through our neighborhood, large letters emblazoned on its front reading "Here comes Grossman!" and on its back "There goes Grossman!" With the best of intentions, we could not help recalling the image as we recently came across a courageous, forthright statement of position on the part of David Grossman, President of the Los Angeles Society of Clinical Psychology, in *LASCP News*.

Speaking in defense of "service-oriented clinical psychologists," President Grossman jabbed hard at what he felt were inequities, plugged equally hard for action "to counteract certain trends which are deemed detrimental and initiate new ones which will help service-oriented clinical psychology to

grow and take its place as a fully mature behavioral science." The LASCP President left his members with few doubts as to where he stood as he remarked at one point:

> We have been partially abandoned by our own family, the academic psychologists; we lack a proper setting in which to forge our professional identity (compared to an academic or research identity); our numbers have suddenly increased—and like all young adolescents we are awkward and ambivalent about our newly won independence, and the newness of our field leaves us with few guidelines.
>
> Another crucial problem needs singling out. The university people who select new graduate students are less interested in service-oriented than academically-oriented people. This means that the type of personality best suited for the "helper" role is selected out. Again I say, this is their prerogative. The pessimistic note is [that] this will lead to a gradual destruction of the very ingredient that is our very uniqueness—the humanistically-oriented, social service-inclined practitioner, be he in private practice or in an institutional setting.

Grossman did not shrink from proposing positive steps: closer liaison with universities, volunteering of community services, recruitment of good young people into the profession, and strong support of the state psychological association. And as he looked to the near future, he could envision

> the beginning of a new era—a professional school for clinical psychology (not just psychotherapy). For such a school to be invited to join a university or state college program would not be unprecedented.

"So, now I have said my piece," concluded the LASCP President, as he finished by adding: "Tear away."

We believe that he means it and that some will.

This Side of the Law. The game is not over till the last man in the ninth is out, and apparently not even then in the case of certification laws. Within the past 12 months, the constitutionality of the Maryland Certification Act has been challenged in the Maryland courts. We violate some laws of the drama by disclosing immediately that the act survived, and we thank Jacob J. Stein for informing us of the details.

According to the Stein account, action was brought by plaintiffs who had been denied certification, with the State and the Maryland Psychological Association in the role of defendants. Because of the crucial issue at stake, MPA saw fit to enlist legal talent of its own to complement the services of the Maryland Attorney General, whose lot it was to defend the act.

The first hearing, in the lower court of Maryland's two-level system, took place on October 19, 1959, with the plaintiffs asserting that since the act was concerned solely with protection of the title "Psychologist" and stopped short of licensing, it represented an unconstitutional deprivation of property rights. On the contrary, MPA Attorney Joseph Sherbow pointed out, the very fact that the legislation had confined itself to the certification procedure was in itself evidence that the defendants were highly cognizant of others' rights and concerned about not excluding anyone unjustly. The presiding judge held decisively in favor of the defendants.

Nonetheless, Stein informs us, the decision was appealed; the case was heard in the Court of Appeals on March 17, 1960, where the act was again upheld, the Court affirming the right of the Legislature to exercise as much restraining power as it deemed necessary in the public welfare at a particular time. Further appeal would involve the United States Supreme Court; it has not been sought.

As Stein puts it in retrospect:

> Each of Maryland's court victories has been an important precedent in this area of legislation. APA generously gave counsel and financial assistance. The legal fees for the two court defenses cost $1,500.00. APA contributed half. In so having weathered these court tests, it appears that Maryland's Certification Act has gained strength and durability, which we hope can be utilized by other states as needed.

The White House. We have reference right now not to its occupant (though we shall vote!) but to its January Conference on Aging for which psychologists have, hopefully, been planning at the state level. In its inner circles psychology is well represented. Serving as Consultant is Robert J. Havighurst, Past President of the Division on Maturity and Old Age; Chairman of the Committee on Social Science and Psychological Research in Gerontology is John E. Anderson; and heading the Committee on the Role and Training of Professional Personnel is Wilma T. Donahue.

All this, we reason, guarantees that whether the White House Conference recommendations get carried to Dick or to Jack, they will, in any case, be psychologically sound.

—JOSEPH M. BOBBITT
Chairman
Board of Professional Affairs
ERASMUS L. HOCH
Administrative Officer
State and Professional Affairs

Notes and News

Deadline for Annual Convention. For the 1961 Annual Convention the deadline for receipt of symposia suggestions is February 15, 1961; for abstracts and summaries of papers it is March 1, 1961; and for fully organized symposia it is March 15, 1961. Details will be in the "Call for Papers and Symposia" in the December issue. Members are invited to submit topics for symposia to be sponsored by the APA Convention Committee to: Alexander G. Wesman, Chairman; 1333 Sixteenth Street N.W.; Washington 6, D. C.

The **American Board of Examiners in Professional Psychology** is pleased to announce the award of its diploma to additional members of the profession. Awards have been made to the following 86 candidates who have satisfactorily completed both written and oral examinations in addition to all other requirements of training, experience, and endorsements:

CLINICAL PSYCHOLOGY

Irving Alexander
Melvin Allerhand
Paul Baer
Robert W. Baker
Morton Bard
Arthur J. Bindman
Leonard Blank
Frank Boring
Guinevere Chambers
Richard Clampitt
Jonathan Cummings
Richard Dana
Paul Daston
Nicholas P. Dellis
Andrew S. Dibner
Herbert Dörken
J. Wilbert Edgerton
Harold J. Fine
Edwin Fishbaine
Samuel C. Fulkerson
Harold Gilberstadt
David S. Goodenough
David C. Goodrich
Jules B. Grossman
Emanuel E. Hammer
William E. Harris
Paul Hauck
James L. Hedlund
Alfred Heilbrun
Charles Heineman
Norman B. Henderson
Nicholas Hobbs
Wilma Inskip
Carroll Izard
Laverne C. Johnson
Robert L. Kahn
Harry I. Kalish
John R. Kleiser
Maurice G. Kott
Isadore Krasno
Philip E. Kubzansky
Martin Lakin
Ernest S. Lawrence
Irving Leiden
Harry Levinson
James E. Lindemann
Stanley Lofchie
Winafred B. Lucas
Richard Lundy
William Lysak
James L. McCary
Archer Michael
Ann S. Miller
Lovick Miller
Melvin Muroff
Sherman Nelson
Forrest C. Orr
James W. Parker
Oscar Parsons
Frank J. Pizzat
Sidney D. Prince
Kenneth Purcell
Stephen S. Rauch
S. Stansfeld Sargent
Donald P. Schmidt
Rowland H. Shepard
William G. Shipman
Harold Shulman
Saul M. Siegel
Jacob O. Sines
Carl N. Sipprelle
Dorothy Stock
John W. Sutton
Maurice K. Temerlin
Sven F. Wahlroos
Leopold O. Walder
Wirt M. Wolff

COUNSELING PSYCHOLOGY

Kenneth Eells
Forrest Erlandson

INDUSTRIAL PSYCHOLOGY

William B. Askren
Mortimer Feinberg
Benjamin Fruchter
William E. Jaynes
Donald Livingston
Herbert H. Meyer
Hobart Osburn

According to ABEPP policy, all previous awards have been announced in the *American Psychologist*. To date, ABEPP has made a total of 1,550 awards of its diploma. These awards are distributed as follows:

Diploma awarded to senior members of the APA and the Canadian Psychological Association with waiver of written and oral examination	1,086
Diploma awarded to members of the APA and the Canadian Psychological Association by satisfactory performance on written and oral examinations	464
	1,550

ABEPP is now accepting applications for admission to its 1961 written examination. Applications will be processed without delay, will be reviewed when all necessary materials have been assembled, and candidates will be informed concerning admission. In meeting the postdoctoral experience requirement of 4 years, ABEPP will count experience to December 31 of the year in which the applicant wishes to register for written examination. The final date for making application for written examination in 1961 is **March 1, 1961.** Prospective candidates should take note of this change in date. ABEPP has prepared a statement entitled *Policies and Procedures*. This pamphlet gives specific information on requirements for candidacy, fields of certification, the nature of acceptable qualifying experience, and evaluative procedures, including written and oral examinatons and policies governing these examinations. Requests for information

should be addressed to: Noble H. Kelley, Secretary, American Board of Examiners in Professional Psychology; Southern Illinois University; Carbondale, Illinois.

The names of the following members of APA were omitted from the biographical section of the 1960 *Directory:*

Crane, Lois H. 1330 Marble Road, East Lansing, Michigan. A(30); M(58) 7, 12.

Davis, R. C. Psychology Department, Indiana University, Bloomington, Indiana. F(34) 3.

Orlinsky, Nancy. Neuropsychiatric Institute, Psychology Department, 912 South Wood Street, Chicago 12, Illinois. A(57); M(58) 8, 12.

Otis, Arthur S. 820 28 Avenue South, St. Petersburg 5, Florida. F(21); L(57).

Prevatta, Paul. Maryland Training School for Boys, 2400 Cub Hill Road, Baltimore 34, Maryland. A(59).

Strand, Marguerite D. Secondary Education Department, San Diego State College, San Diego 15, California. A(48); M(58).

Turner, William D. School of Social Work, University of Pennsylvania, 2410 Pine Street, Philadelphia 3, Pennsylvania. A(29); F(39).

Vris, Thomas. Personnel Research Division, Prudential Insurance Company, Newark, New Jersey. A(56); M(58).

Wauters, Helen L. 701 High Street, Auburn, California. A(48); M(58).

Yaffe, Paul. 3204 Milford Avenue, Baltimore 7, Maryland. A(57); M(58).

The American Institute for Research announces the addition of the following psychologists to the research staff of the Los Angeles office:

Harry E. Anderson, Jr., Research Scientist, formerly with the System Development Corporation

Evan W. Pickrel, Senior Research Scientist, formerly with the RAND corporation

Alec J. Slivinske, Senior Research Scientist, formerly with HRB-Singer, Inc.

Ira Belmont has been appointed a Research Associate in the Department of Experimental Psychiatry at Hillside Hospital, Glen Oaks, New York.

Viola Bloom, formerly at Nebraska Psychiatric Institute, has been appointed Associate Professor of Psychology at Elmhurst College.

Lawrence P. Blum has been appointed Associate Professor of Education at the University of Wisconsin-Milwaukee.

In the Department of Psychology at Bucknell University:

Richard Teevan, formerly at Smith College, has been appointed an Associate Professor.

Douglas Candland, formerly at the University of Virginia, has been appointed an Assistant Professor.

John Braun has resigned to accept a position at Texas Christian University.

Robert H. Cassel, formerly at the Dixon State School, has been appointed Chief of Psychological Services at the State Colony and Training School, Pineville, Louisiana.

The Psychology Department at the Elmhurst City Hospital, New York, includes: **Ann Neel** and **Max Prola,** Psychologists; **Stuart Weissman,** Psychologist Intern; **Reuben Fine** and **Milton Kapit,** Visiting Consulting Psychologists; and **Gerhart H. Saenger,** Visiting Lecturer in Social Psychology.

Harold Kenneth Fink has been selected as Field Counselor for Broward County, Florida, by the Scientific Marriage Foundation.

Alfred R. Fregly has resigned from the Escambia Guidance Clinic to enter private practice in Gulf Breeze, Florida.

George Genn has been appointed Professor of Education at Paterson State College.

Bertram Gold is Vice-President of the recently established Creative Research Services, Inc. in New York City to provide market research and consulting services for industry and advertising agencies.

L. M. Gustafson, formerly at Oklahoma State University, has been appointed an Associate Professor in the Department of Psychology at the University of Oklahoma.

Max L. Hillmer, Jr. has been appointed Research Director in the School of Nursing at the University of Washington.

Walter V. Clarke Associates, Inc. announces the addition to its staff of **A. Edward Hoffman** as Account Associate in the New England area.

Glen D. Jenson is now an Assistant Professor in the Department of Psychology at the University of New Mexico.

James G. Kelly has been appointed a Scientist in the Active Reserve and **John R. Newbrough** a Senior Assistant Scientist in the Regular Corps of the United States Public Health Service Commissioned Officer Corps; both have been assigned to the Mental Health Study Center, Langley Park, Maryland.

David Kirschner, formerly with the East Williston Public Schools, has now entered the full-time private practice of clinical psychology in North Merrick, Long Island.

Eda J. LeShan, formerly at the New Rochelle Guidance Center, is now Educational Director for the Manhattan Society for Mental Health.

Lloyd Lofquist has been appointed Assistant Chairman of the Department of Psychology at the University of Minnesota.

The Psychology Department at Long Island University (Zeckendorf Campus) announces the following additions to the staff: **Robert Neel,** Associate Professor; **William D. Katz**, Associate Professor; **James Ray Adams**, Instructor; **Arthur Lefford,** Adjunct Assistant Professor; and **Solomon Machover,** Adjunct Professor.

Richard C. McKee has been appointed Dean of Personnel at Eastern New Mexico University.

Psychological Research Associates, Inc. announces the change of its corporate name to the Matrix Corporation, with two operating divisions: Psychological Research Associates and Man-Machine Systems.

Bernard I. Murstein, formerly at the University of Portland, has been appointed Director of Research at the Interfaith Counseling Center, Portland, Oregon. He will also work on a National Institute of Mental Health grant investigating the role of the stimulus in the assessment of personality through thematic fantasy techniques.

Roy W. Romberger has been appointed Personel Director of Keuffel & Esser Company, Hoboken, New Jersey.

In the Department of Psychology at St. Louis University:

Wendell S. Phillips, on leave from the Veterans Administration as Visiting Professor of Psychology, will also serve as Director of the department.

Donald Kausler, formerly at the University of Arkansas, has been appointed an Associate Professor.

Allan Barclay, formerly at the St. Louis VA Mental Hygiene Clinic, has been appointed an Assistant Professor and will also serve as Chief Psychologist at Glennon Memorial Hospital for Children.

Donald Gene Davenport, formerly at the University of Minnesota, and **Fred Thumin,** formerly with the Gardner Advertising Company, have been appointed Assistant Professors.

Francis Harmon has accepted a research position with the Naval Bureau of Personnel in Washington, D. C.

Edward Hackett is now with Staff Research, Chief of Naval Air Technical Training, Naval Air Station, Memphis, Tennessee.

Henry L. Sisk has accepted a position as Associate Professor of Management in the School of Business Administration at North Texas State College and the position of Adjunct Professor of Management in the School of Business at Texas Christian University.

B. F. Skinner, of Harvard University, received an honorary Doctor of Science degree from North Carolina State College.

William Sloan, formerly at the Pineville State Colony and Training School, has accepted the position of Superintendent of the Austin State School, Texas.

Thomas F. Staton received an honorary award from the federal government for his service at the Air University's Command and Staff College. Staton is currently Head of the Department of Psychology at Huntingdon College.

Albert S. Thompson, of Teachers College, Columbia University, will spend the academic year in Ghana, West Nigeria, and at the University of London as an exchange Professor in the Afro-Anglo-American Cooperative Program in Teacher Education.

Marvin Zuckerman has been appointed an Assistant Professor in the Psychology Department at Brooklyn College.

The following roster of officers has been announced:

Mississippi Valley Psychological Association
President: William O. Hambacher
President-elect: Renate G. Armstrong
Secretary: George Mally
Treasurer: Catherine W. Davis

The **American Institute for Research** announces a program of annual Creative Talent Awards to identify and encourage creative talent in psychology and related fields—especially to encourage new, original, and provocative ideas and methods which will contribute to the advancement of the science of human behavior. Outstanding dissertations will be selected annually by three panels. From among the three dissertations selected, the one judged as showing the most promise for creative contributions to scientific knowledge will receive an award of $1,000. The other two dissertations selected by the panels will receive awards of $500 each. Citations of honorable mention will be made in each of the three fields for dissertations found worthy of national recognition for competent and creative work. Dissertations completed during the period 1 July 1960–31 August 1961 will be eligible for the first annual awards. For the first year, awards will be offered in the three fields: Perception, Learning, and Motivation; Development, Counseling, and Mental Health; and Measurement and Evaluation: Individual and Group Behavior. Further information about the program may be obtained by writing to: American Institute for Research; 1808 Adams Mill Road, N.W.; Washington 9, D. C.

The *Division of Biological and Medical Sciences* of the **National Science Foundation** announces that the next closing date for receipt of basic research proposals in the life sciences is January 15, 1961. Proposals received prior to that date will be reviewed at the spring meetings of the foundation's advisory panels, and disposition will be made approximately four months following the closing date. Proposals received after the January 15, 1961 closing date will be reviewed following the summer closing date of May 15, 1961. Inquiries should be addressed to: National Science Foundation; Washington 25, D. C.

Scientists and science teachers in colleges, universities, and nonprofit organizations are invited by the National Science Foundation to submit proposals for the development of prototypes of new laboratory equipment for use in the nation's schools and colleges. Proposals judged most meritorious will be supported by the foundation under a program designed to encourage the development of inexpensive and practical apparatus for use in the laboratory phases of science instruction. Although proposals may be submitted at any time, those to be considered for support during the current fiscal year should be sent before December 15, 1960 to: Course Content Improvement Section, *Division of Scientific Personnel and Education*, National Science Foundation; Washington 25, D. C.

Support for advanced study in the Doctoral Program in Social Work and Social Science offered at the **University of Michigan** is available through grants from the National Institute of Mental Health and the Russell Sage Foundation. Stipends range from $750 to $4,350. Fellowship applications will be received up to February 1, 1961. For detailed information and application forms, write to: Henry Meyer; School of Social Work, University of Michigan; Ann Arbor, Michigan.

Research projects recently contracted for under the **Cooperative Research Program** of the United States Department of Health, Education, and Welfare include:

Roscoe A. Boyer, University of Mississippi, "The Use of Mathematical Programing to Solve Certain Problems in Public School Transportation"

Julian C. Stanley, University of Wisconsin, "Development and Analysis of Experimental Designs for Ratings"

Charles D. Smock, Purdue University, "Effects of Motivational Factors on Perceptual-Cognitive Proficiency of Children Who Vary in Intellectual Level"

Bryce B. Hudgins and Louis M. Smith, Washington University, "Effectiveness and Efficiency of Problem-Solving Laboratory Groups and Classroom Groups as Functions of Selected Individual and Group Variables"

Wallace H. Maw, University of Delaware, "The Measurement of Curiosity in Elementary School Children"

Leonard D. Goodstein, State University of Iowa, "Personality Correlates of Academic Adjustment"

Mildred C. Templin, University of Minnesota, "The Identification of Kindergarten Children Least Likely to Show Spontaneous Improvement in Speech Sound Articulation"

Claude B. Elam, Texas Christian University, "Inductive Concept Formation in Normal and Retarded Subjects"

William W. Farquhar, Michigan State University, "A Comprehensive Study of the Motivational Factors Underlying Achievement of Eleventh-Grade High School Students"

Wilbert J. McKeachie, University of Michigan, "Research on the Characteristics of Effective College Teaching"

G. Orville Johnson, Syracuse University, "Relationship between Perception and Learning in the Mentally Retarded"

Louis A. Fliegler, Syracuse University, "Systematic Variation of Certain Conditions Related to Learning in the Mentally Retarded: Reinforcement"

Gabriel M. Della-Piana: University of Utah, "An Experimental Evaluation of Machine Teaching: The Effect of Certain Learning Outcomes of Variations in Motivational Characteristics of the Learner and the Form of the Learner's Responses"

Pauline S. Sears, Stanford University, "The Effect of Teaching Methods on the Strength of Achievement Motive and Work Output of Elementary School Children"

Milton Rokeach, Michigan State University, "The Nature of Analysis and Synthesis and Some Conditions in the Classroom Which Facilitate or Retard These Cognitive Processes"

Edgar L. Lowell and Mary F. Woodward, University of Southern California, "A Linguistic Approach to the Education of Aurally Handicapped Children"

Martin Deutsch, New York Medical College, "Communication of Information in Elementary Classrooms"

Walter T. Plant and Charles W. Telford, San Jose State College, "The Psychological Impact of the Public Two-Year College on Certain Nonintellectual Functions"

Ralph J. Garry, Boston University, "The Nature and Formation of Spatial Concepts in Congenitally Blind Children"

W. W. Charters, Jr., Washington University, "Teacher Perceptions of Administrator Behavior"

John W. M. Rothney, University of Wisconsin, "The Discovery and Guidance of Superior Students"

Merle M. Ohlsen, University of Illinois, "Improved School Adjustment of Underachieving Gifted Fifth Graders"

Merle M. Ohlsen, University of Illinois, "Response Patterns Associated with Group Counseling"

Philip J. Runkel, University of Illinois, "Changes in Schools Which Do and Do Not Send Staff Members to Training Institutes in Counseling"

Ralph F. Berdie, University of Minnesota, "Trends in Post-High School Plans over an Eleven-Year Period"

Wallace A. Kennedy, Florida State University, "Standardization of the Third Revision of the Stanford-Binet Intelligence Scale on Negro Elementary School Children in the Southeastern United States"

James J. Gallagher, University of Illinois, "Productive Thinking of Gifted Children"

Bernard B. Schlanger, West Virginia University, "The Effects of Listening Training on the Auditory Thresholds of Mentally Retarded Children"

Harold Guetzkow, University of Chicago, "Education for Innovative Behavior in Executives"

William G. Perry, Jr., "Patterns of Development in Thought and Values of Students in a Liberal Arts College"

E. Paul Torrance, University of Minnesota, "Understanding the Fourth-Grade Slump in Creative Thinking"

John H. Gaeth, Wayne State University, "Verbal and Nonverbal Learning in Children, Including Those with Hearing Losses"

Ray H. Bixler, University of Louisville, "Comparative Comprehension by Blind Children of Braille and Recordings at Different Compressions"

Joseph C. Bledsoe, University of Georgia, "The Self-Concepts of Elementary School Children in Relation to Their Academic Achievement, Intelligence, Interests, and Manifest Anxiety"

Horacio J. A. Rimoldi, Loyola University, "Training in Problem Solving"

Eleanor E. Maccoby and Lucy Rau, Stanford University, "Differential Cognitive Abilities"

Thomas L. Hilton, Carnegie Institute of Technology, "Cognitive Processes in Career Decision Making"

Arthur W. Staats, Carolyn K. Staats, and Richard E. Schutz, Arizona State University, "Textual Behavior and Its Function in Communication"

William A. Owens, Purdue University, "Life History Correlates of Age Changes in Mental Abilities"

Raymond C. Hummel, Harvard University, "An Evaluation of a Model for Guidance Counseling"

Billey Levinson and Hayne W. Reese, University of Buffalo, "The Development of Discrimination Learning Set in Preschool Children, Fifth Graders, College Freshmen, and the Aged"

Philip R. Merrifield, University of Southern California, "Aptitude and Personality Measures Related to Creativity in Seventh-Grade Children"

Martha T. Mednick and Sarnoff A. Mednick, University of Michigan, "The Associative Basis of the Creative Process"

Robert D. Hess and David Easton, University of Chicago, "The Development of Basic Attitudes and Values toward Government and Citizenship during the Elementary School Years"

Bruce J. Biddle, University of Missouri, "Explorations in Teacher Role"

C. Robert Pace, Syracuse University, "The Influence of Academic and Student Subcultures in College and University Environments"

Evan R. Keislar, University of California, "Abilities of First-Grade Pupils to Learn Mathematics in Terms of Algebraic Structures by Means of Teaching Machines"

Robert L. Thorndike, Teachers College, Columbia University, "The Concepts of Over- and Under-Achievement"

Celeste McCollough has been awarded a USPHS grant for a study of **involuntary responses**: heart, breathing, and pupillary responses in the cat to tones paired with shock or light.

The Staten Island Mental Health Society is building a new research wing under a USPHS Research Facilities Grant. This new division was founded to undertake research in clinical and community aspects of **mental health programs** under a 3-year grant from the Avalon Foundation. Wallace Mandell, formerly with the Texas State Health Department, has been appointed Director of Research.

The United States Office of Education has awarded Wendell Smith and J. W. Moore, of Bucknell University, a grant for programing and evaluating **self-teaching materials** in mathematics, and

a grant for a study of the "Size-of-Step and Cueing in Programing Spelling Materials."

The SRA Test Department announces publication of four 1960 **technical monographs** on: National Merit Scholarship Qualifying Test, SRA High School Placement Test, Army General Classification Test, and Tests of General Ability (John Flanagan). Professional complimentary copies of these publications may be obtained from: Test Department, Science Research Associates; 259 East Erie Street; Chicago 11, Illinois.

The Department of Psychology at the University of New Mexico has expanded its staff and graduate program and is now offering the **PhD in general psychology.**

The Psychological Service Center, Teaneck, New Jersey, announces the opening of the **Speech Clinic** with services available to children and adults for the diagnosis and treatment of voice and language disorders.

On October 4, 1960, United Cerebral Palsy of New York City and the Institute for the Crippled and Disabled (23 Street and Third Avenue; New York 10, New York) initiated a **Training Workshop for the Cerebral Palsied** to develop new and improved sheltered workshop techniques for the cerebral palsied.

On October 20, 1960, a multidisciplinary symposium on **"Imprinting"** was held at Dorothea Dix Hospital, Raleigh, North Carolina.

The Annual Meeting of the **National Association for Mental Health** (10 Columbus Circle; New York 19, New York) will be held in Denver, Colorado, on November 17–19, 1960.

"The Development of Patterns of Affection" is the topic of the twenty-eighth **Thomas William Salmon Lecture** to be given by Harry F. Harlow on December 5, 1960 at the New York Academy of Medicine.

The second residential workshop on **"Introduction to Analytical Psychology for Clinicians"** will be held at Pacific Grove, California, on June 3–15, 1961. For application forms and further information, write to: Department of Social Sciences, University Extension, University of California; Los Angeles 24, California.

Members of APA who are planning to visit **Britain** are invited to notify the Editor of the *Bulletin* of the British Psychological Society so that their visit may be announced in the "Visitors" section. The *Bulletin* is circulated to all members of the British Psychological Society. It is suggested that the following information should briefly be given: visitor's name and position, purpose and approximate duration of the visit, institution or other address while in Britain. Since the *Bulletin* appears only three times a year, it would be helpful if this information were sent as early as possible. It should be addressed to: Editor, *Bulletin*, British Psychological Society; Tavistock House South, Tavistock Square; London, W.C.1., England.

The Social Science Research Council has recently announced that it will support travel to the fourteenth **International Congress of Applied Psychology,** Copenhagen, August 13–19, 1961. *The closing date for receipt of applications has been extended to January 9, 1961.* Application forms must be obtained from: Social Science Research Council; 230 Park Avenue; New York 17, New York.

Convention Calendar

American Psychological Association: August 31–September 6, 1961; New York, New York

For information, write to:

Janice P. Fish
American Psychological Association
1333 Sixteenth Street, N.W.
Washington 6, D. C.

Southern Society for Philosophy and Psychology: March 30–April 1, 1961; Atlanta, Georgia

For information, write to:

Dan R. Kenshalo
Florida State University
Tallahassee, Florida

Eastern Psychological Association: April 7–8, 1961; Philadelphia, Pennsylvania

For information, write to:

Carl H. Rush
P. O. Box 252
Glenbrook, Connecticut

Deadline for papers: December 2, 1960

Southeastern Psychological Association: April 13–15, 1961; Gatlinburg, Tennessee

For information, write to:

Susan W. Gray
Box 30
George Peabody College for Teachers
Nashville 5, Tennessee

Midwestern Psychological Association: May 4–6, 1961; Chicago, Illinois

For information, write to:

I. E. Farber, Secretary-Treasurer
Midwestern Psychological Association
Department of Psychology
State University of Iowa
Iowa City, Iowa

American Public Health Association: October 31–November 4, 1960; San Francisco, California

For information, write to:

Berwyn F. Mattison
1790 Broadway
New York 19, New York

American Association for the Advancement of Science: December 26–31, 1960; New York, New York

For information, write to:

Raymond L. Taylor
American Association for the Advancement of Science
1515 Massachusetts Avenue, N.W.
Washington 5, D. C.

JOURNAL OF INDIVIDUAL PSYCHOLOGY

VOLUME 16 NOVEMBER, 1960 NUMBER 2

CONTENTS

Published semi-annually

Subscription Price $4.00 Single Copies $2.50

Order from:

Journal of Individual Psychology, University of Vermont, Burlington, Vermont

PUBLICATIONS OF THE JAPANESE PSYCHOLOGICAL ASSOCIATION

The Japanese Psychological Association established in 1927 and the only representative organization of Japanese psychologists in all fields, publishes two journals, the JAPANESE JOURNAL OF PSYCHOLOGY and the JAPANESE PSYCHOLOGICAL RESEARCH.

JAPANESE PSYCHOLOGICAL RESEARCH

A Quarterly Journal of Psychology, printed in English.

Issued in April, June, September and December.

Contents

Volume 2, No. 1

T. INDOW & T. KAWAI
A construction of uniform lightness scales on various backgrounds in unified terms.

T. OYAMA & R. NANRI
The effects of hue and brightness on the size perception.

T. MORI & F. OGITANI
An experimental study of transposition of discrimination learning in the white rat.

C. SATO
Orientation in the perception of space.

News and notes

Volume 2, No. 2

Y. MATSUMIYA
The effects of US intensity and CS-US pattern on conditioned emotional response.

Y. UMEOKA & Y. TAKADA
Goal-oriented group process in an operationally constrained situation (I).

T. SUGIMURA
Discrimination shifts as a function of degree of original learning and materials in the original and shifted discrimination tasks.

T. HIDANO & S. SHIBA
Reaction time and redundancy of stimulus sequence.

Subscription price $5.00 per year
$4.00 APA member subscription

THE JAPANESE JOURNAL OF PSYCHOLOGY

A Representative Journal of Psychology in Japan, including Summaries in English, German, or French.

Bimonthly.

Contents

Volume 31, No. 1

N. KITAO
Effects of mediated association on paired-associate learning: I.

Y. MATSUYAMA
Drive discrimination learning by pain and fear.

U. KUNO
A probability model for verbal learning.

T. SOMIYA
Adient reaction potential and abient reaction potential as a function of the distance from the object (1).

T. OYAMA & J. NAKAHARA
The effects of lightness, hue and area upon the apparent transparency.

Minor studies

Book reviews and notes

Volume 31, No. 2

F. MARUI
A normative study on TAT . . . Chiefly on emotional tone, outcome and shift.

K. FUJISAWA & T. OBONAI
The psycho-physiological studies of hypnotic sleep.

K. ITO
Factorial studies on the work curve of Uchida-Kraepelin psychodiagnostic test.

Minor studies

Book reviews and notes

Subscription price $4.00 per year

SEND SUBSCRIPTION ORDERS TO:

JAPANESE PSYCHOLOGICAL ASSOCIATION
c/o The Faculty of Letters
University of Tokyo
Tokyo, Japan

SECRETARIES OF APA DIVISIONS

1. Division of General Psychology
   Dr. Lawrence M. Stolurow
   Department of Psychology
   University of Illinois
   Urbana, Illinois

2. Division on the Teaching of Psychology
   Prof. T. L. Engle
   Indiana University
   Fort Wayne Center
   1120 South Barr Street
   Fort Wayne 2, Indiana

3. Division of Experimental Psychology
   Dr. Frederick A. Mote
   Department of Psychology
   University of Wisconsin
   Madison 6, Wisconsin

5. Division on Evaluation and Measurement
   Dr. Roger T. Lennon
   World Book Company
   Tarrytown, New York

7. Division on Development Psychology
   Dr. Marian Radke-Yarrow
   National Institute of Mental Health
   Building 10, Room 2N–208
   Bethesda 14, Maryland

8. Division of Personality and Social Psychology
   Dr. Robert L. French
   Psychological Corporation
   304 East 45 Street
   New York 17, New York

9. Society for the Psychological Study of Social Issues—A Division of the APA
   Dr. Elizabeth M. Douvan
   Survey Research Center
   University of Michigan
   Ann Arbor, Michigan

10. Division on Esthetics
    Dr. Henry Gleitman
    Swarthmore College
    Swarthmore, Pennsylvania

12. Division of Clinical Psychology
    Dr. Sol L. Garfield
    Nebraska Psychiatric Institute
    602 South 44 Avenue
    Omaha 5, Nebraska

13. Division of Consulting Psychology
    Dr. Ruth Bishop Heiser
    10 East Sharon Avenue
    Glendale, Ohio

14. Division of Industrial and Business Psychology
    Dr. Brent N. Baxter
    Prudential Insurance Company
    763 Broad Street
    Newark, New Jersey

15. Division of Educational Psychology
    Prof. Julian C. Stanley, Jr.
    305 South Owen Drive
    Madison 5, Wisconsin

16. Division of School Psychologists
    Dr. Keith J. Perkins
    6310 North 14 Place
    Phoenix, Arizona

17. Division of Counseling Psychology
    Dr. Dorothy M. Clendenen
    Psychological Corporation
    304 East 45 Street
    New York 17, New York

18. Division of Psychologists in Public Service
    Mr. Luigi Petrullo
    2431 North Edgewood Street
    Arlington, Virginia

19. Division of Military Psychology
    Dr. Philip I. Sperling
    6108 Augusta Drive
    Springfield, Virginia

20. Division on Maturity and Old Age
    Dr. Jack Botwinick
    Section on Aging
    National Institute of Mental Health
    Bethesda 14, Maryland

21. Society of Engineering Psychologists—A Division of the APA
    Dr. Harry J. Older
    Psychological Research Associates
    507 18 Street South
    Arlington 2, Virginia

22. National Council on Psychological Aspects of Disability—A Division of the APA
    Dr. Leonard Pearson
    Counseling Center
    Central YMCA
    19 South LaSalle Street
    Chicago 3, Illinois

23. Division of Consumer Psychology
    Dr. Dik W. Twedt
    Faison & Twedt, Inc.
    41 East Oak Street
    Chicago 11, Illinois